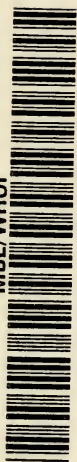


CLASSICS OF
MODERN SCIENCE

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CLASSICS OF
MODERN SCIENCE

THERE is no grander nor more intellectually elevating spectacle than that of the utterances of the fundamental investigators in their gigantic power. Possessed as yet of no methods—for these were first created by their labors and are only rendered comprehensible to us by their performances—they grapple with and subjugate the object of their inquiry and imprint upon it the forms of conceptual thought.

—ERNST MACH

78

CLASSICS OF MODERN SCIENCE

(COPERNICUS TO PASTEUR)

EDITED BY

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OF THE SOUTH · EDITOR, THE
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MANUFACTURED

IN THE UNITED STATES OF AMERICA

TO MY FORMER ASSOCIATES OF THE FACULTY,
AND THE STUDENTS OF THE NEW YORK
STATE COLLEGE OF FORESTRY AT SYRACUSE
UNIVERSITY



PREFACE

"The history of science," wrote Du Bois-Reymond, "is the real history of mankind." Gradually we are coming to realize the significance of that statement, and the sooner we realize it on a grand scale the more shall we hasten the happiness of man.

Fortunately for education, science no longer has to fight for its inclusion among the courses offered for study in colleges and universities. As scientific knowledge increases and the technique of teaching science improves, the exact knowledge of the few more rapidly becomes the accepted knowledge of the many. More than that, the scientific attitude of mind produces many of the virtues which in old-fashioned courses in ethics were taught as objectively as a problem in geometry. Patience, endurance, humility, teachableness, honesty, accuracy—without these it is impossible for a scientist properly to work. And the history of science is as inspiring in its human values as are the legends of the saints. Contemplate the heroism of a Galileo, the patience of a Darwin, the humility of a Pasteur; a modern eleventh chapter of *Hebrews* might be written listing the names of all those men of faith who by quiet work, unremitting in their zeal, one by one discovered facts which have made man's lot easier and happier in what was otherwise to him a hostile and unhappy universe.

Little by little, accretion upon accretion, man's knowledge of the physical forces of his universe has been increased, but his progress has often been retarded by those who, with good intentions, superstitiously feared the power of the gods who, as in the story of Brunhilde, encircled their mysteries with a ring of fire. Periodically superstition re-arises, but it does not permanently halt the advance deploy of armed skirmishers, however much it may temporarily retard the advancement of knowledge. Since the seventeenth century, however, so remarkable has been the progress of science, so evident have been its beneficent achievements, that regardless of the present assault upon one phase of science, western civilization is committed to this way of discovery. But it is no easy way! "The rapid increase of natural

knowledge," wrote Thomas Henry Huxley, "which is the chief characteristic of our age, is affected in various ways. The main army of science moves to the conquest of the new worlds slowly and surely, nor ever cedes an inch of the territory gained. But the advance is covered and facilitated by the ceaseless activity of clouds of light troops provided with a weapon—always efficient, if not always an arm of precision—the scientific imagination. It is the business of these *enfants perdus* of science to make raids into the realms of ignorance wherever they see, or think they see, a chance; and cheerfully to accept defeat, or it may be annihilation, as the reward of error. Unfortunately the public, which watches the progress of the campaign, too often mistakes a dashing incursion . . . for a forward movement of the main body; fondly imagining that the strategic movement to the rear, which occasionally follows, indicates a battle lost by science."

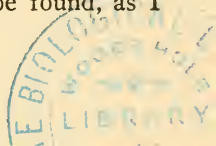
It is regrettable that Huxley was compelled to use the metaphor of a battle in describing the general advance of scientific knowledge; how much better it would have been if he could have used a scientific word like *enzyme* or *catalyst* in referring to those courageous men of the laboratory and the field who went forth alone with instruments to discover things as they really are and changed fields of knowledge through their discoveries. But if he had employed these scientific terms, no one, apart from the select company of scientists themselves who have had to evolve a special language of their own to express new matters and new meanings, would understand him. People who use strange tongues are always suspect to the populace. If science is to be "understood" by the people, the people's language must be used. Fortunately, for the sake of science, scientists themselves are now keenly aware of the necessity of presenting their findings in language which may be understood by the ordinary man. Huxley himself made the *liaison* in his age, an age in which battles were highly idealised. His grandson, however, speaking to our age, rephrases the idea in a mode more acceptable to us: "Each science or branch of science seems roughly to go through three main phases in its development. There is first a preliminary phase in which miscellaneous sporadic knowledge is amassed and is dated; theories are pursued, often to be proved valueless. There then comes a classic or heroic age, in which a general principle of firmly interrelated princi-

ples is gradually laid down, upon which in its turn a coherent architecture of theory can be built, and finally this passes over into a period of maturity, in which the position is consolidated, the scope of the principles widened, their bases more finally tested, and their consequences worked out in fullest detail. Naturally, each stage lasts for a considerable time, and in many cases a science which thought itself securely embarked upon the third phase is reminded by some fundamental discovery that it is still only in the second."¹

These movements of science have produced a copious literature which has not enjoyed the same attention and reading as imaginative books, because, once the ideas are known and incorporated into the existing body of scientific knowledge, these scientific writings tend to acquire chiefly an historical interest. Yet they are monuments of the advance of civilization, and deserve a better fate. Many of them are still interesting to read as human documents because they illustrate how painfully and slowly man's exact knowledge of verifiable phenomena has been accumulated. No one outside of the small company of highly trained scientists can read all of them through, yet most of them have sections which are as readable and as exciting as any modern novel. It is the purpose of this book to present to the young college student and to the general reader some of the more representative of these classics in the literature of science, bringing together in this convenient form at least some reminders of a vast field of reading where one may browse for a lifetime with interest and profit. If it be used in conjunction with a history of science it will readily supply a vivid sense of the movement of the mind of western civilization, increasing in us a respect for the effort of our ancestors, and inspire us to encourage and to forward the work of contemporary scientists, and restrain us at least from hindering them in their efforts.

Although the selections may be used as a textbook in courses like Introduction to Modern Civilization, Philosophy, and The History of Science now given in the more progressive colleges and universities, it may also profitably be used as a text for freshman or sophomore readings in English courses given in colleges predominantly technical or scientific, like Engineering, Agricultural, and Forestry Colleges. In those English courses where emphasis upon ideas is made to provide the inspiration for writing, these selections will be found, as I

¹ Julian Huxley, in *Harper's Magazine* for April, 1926.



found them in my own work, to stir up considerable discussion and to provide opportunities for reading modern scientific literature. Moreover, the literary style of science at its best will be found to be excellently illustrated in these straightforward, coherent sentences written by some of the world's clearest thinkers. They illustrate concretely what Tyndall remarked in his closing words of the famous *Belfast Address*: "It has been said that science divorces itself from literature. The statement, like so many others, arises from lack of knowledge. A glance at the less technical writings of its leaders—of its Helmholtz, its Huxley, and its Du Bois-Reymond—would show what breadth of literary culture they command. Where among modern writers can you find their superiors in clearness and vigor of literary style? Science desires no isolation, but freely combines with every effort toward the bettering of man's estate. Single-handed and supported not with outward sympathy, but by inward force, it has built at least one great wing of the many-mansioned home which man in his totality demands. . . . The world embraces not only a Newton, but a Shakespeare; not only a Boyle, but a Raphael; not only a Kant, but a Beethoven; not only a Darwin, but a Carlyle. Not in each of these, but in all, is human nature whole. They are not opposed, but supplementary; not mutually exclusive, but reconcilable."

WILLIAM S. KNICKERBOCKER

UNIVERSITY OF THE SOUTH
SEWANEE, TENN.

April 5, 1927

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CLASSICS OF
MODERN SCIENCE

FRANCIS BACON

1561-1626

Francis Bacon, Lord Verulam, is distinguished in the history of science for his criticism of the methods of knowledge of his day. In his great writings, "The Advancement of Learning" (1605), "Novum Organum" (1620), and "De Augmentis Scientiarum" (1623), he cumulatively outlined a new method, named after him, whereby all knowledge was referred to experience and corrected by experiment. His inductive method was epoch-making in that it established the technique underlying all modern science.

He was born in London, January 22, 1561, the son of Sir Nicholas Bacon, Lord Keeper of the Seals. In 1573, at the age of twelve, he matriculated in Trinity College, Cambridge. After his father's death, in 1579, he led a precarious life, accumulated many debts, and ended by accusing his intimate friend, Lord Essex, of treason. In 1607 King James appointed him Solicitor. In 1613 he became Attorney General, and in 1618 was made Lord Chancellor and knighted Baron Verulam. The following year he was impeached for bribery, and imprisoned four days for the offense. Thereafter, until his death on April 9, 1626, he gave himself wholly to the development of his new scientific method.

THE METHOD OF INDUCTIVE SCIENCE *

They who have presumed to dogmatize on nature, as on some well investigated subject, either from self-conceit or arrogance, and in the professorial style, have inflicted the greatest injury on philosophy and

* Selection from the Preface to the *Novum Organum*.

learning. For they have tended to stifle and interrupt inquiry exactly in proportion as they have prevailed in bringing others to their opinion; and their own activity has not counterbalanced the mischief they have occasioned by corrupting and destroying that of others. They again who have entered upon a contrary course, and asserted that nothing whatever can be known, whether they have fallen into this opinion from their hatred of the ancient sophists, or from the hesitation of their minds, or from an exuberance of learning, have certainly adduced reasons for it which are by no means contemptible. They have not, however, derived their opinion from true sources, and, hurried on by their zeal and some affectation, have certainly exceeded due moderation. But the more ancient Greeks (whose writings have perished), held a more prudent mean, between the arrogance of dogmatism, and the despair of scepticism; and though too frequently intermingling complaints and indignation at the difficulty of inquiry, and the obscurity of things, and champing, as it were, the bit, have still persisted in pressing their point, and pursuing their intercourse with nature; thinking, as it seems, that the better method was not to dispute upon the very point of the possibility of anything being known, but to put it to the test of experience. Yet they themselves, by only employing the power of the understanding, have not adopted a fixed rule, but have laid their whole stress upon intense meditation, and a continual exercise and perpetual agitation of the mind.

Our method, though difficult in its operation, is easily explained. It consists in determining the degrees of certainty, whilst we, as it were, restore the senses to their former rank, but generally reject that operation of the mind which follows close upon the senses, and open and establish a new and certain course for the mind from the first actual perceptions of the senses themselves. This, no doubt, was the view taken by those who have assigned so much to logic; showing clearly thereby that they sought some support for the mind, and suspected its natural and spontaneous mode of action. But this is now employed too late as a remedy, when all is clearly lost, and after the mind, by the daily habit and intercourse of life, has come prepossessed with corrupted doctrines, and filled with the vainest idols. The art of logic, therefore, being (as we have mentioned) too late a precaution, and in no way remedying the matter, has tended more to confirm errors, than to disclose truth.³ Our only remaining hope and

salvation is to begin the whole labor of the mind again ; not leaving it to itself, but directing it perpetually from the very first, and attaining our end as it were by mechanical aid. If men, for instance, had attempted mechanical labors with their hands alone, and without the power and aid of instruments, as they have not hesitated to carry on the labors of their understanding with the unaided efforts of their mind, they would have been able to move and overcome but little, though they had exerted their utmost and united powers. And just to pause awhile on this comparison, and look into it as a mirror ; let us ask, if any obelisk of a remarkable size were perchance required to be moved, for the purpose of gracing a triumph or any similar pageant, and men were to attempt it with their bare hands, would not any sober spectator avow it to be an act of the greatest madness ? And if they should increase the number of workmen, and imagine that they could thus succeed, would he not think so still more ? But if they chose to make a selection, and to remove the weak, and only employ the strong and vigorous, thinking by this means, at any rate, to achieve their object, would he not say that they were more fondly deranged ? Nay, if not content with this, they were to determine on consulting the athletic art, and were to give orders for all to appear with their hands, arms, and muscles regularly oiled and prepared, would he not exclaim that they were taking pains to rave by method and design ? Yet men are hurried on with the same senseless energy and useless combination in intellectual matters, as long as they expect great results either from the number and agreement, or the excellence and acuteness of their wits ; or even strengthen their minds with logic, which may be considered as an athletic preparation, but yet do not desist (if we rightly consider the matter) from applying their own understandings merely with all this zeal and effort. Whilst nothing is more clear, than that in every great work executed by the hand of man without machines or implements, it is impossible for the strength of individuals to be increased, or that of the multitude to combine.

Having premised so much, we lay down two points on which we would admonish mankind lest they should fail to see or to observe them. The first of these is, that it is our good fortune (as we consider it), for the sake of extinguishing and removing contradiction and irritation of mind, to leave the honor and reverence due to the

ancients untouched and undiminished, so that we can perform our intended work, and yet enjoy the benefit of our respectful moderation. For if we profess to offer something better than the ancients, and yet should pursue the same course as they have done, we could never, by any artifice, contrive to avoid the imputation of having engaged in a contest or rivalry as to our respective wits, excellencies, or talents; which, though neither inadmissible nor new (for why should we not blame and point out anything that is imperfectly discovered or laid down by them, of our own right, a right common to all), yet however just and allowable, would perhaps be scarcely an equal match, on account of the disproportion of our strength. But since our present plan leads us to open an entirely different course to the understanding, and one unattempted and unknown to them, the case is altered. There is an end to party zeal, and we only take upon ourselves the character of a guide, which requires a moderate share of authority and good fortune, rather than talents and excellence. The first admonition relates to persons, the next to things.

We make no attempt to disturb the system of philosophy that now prevails, or any other which may or will exist, either more correct or more complete. For we deny not that the received system of philosophy, and others of a similar nature, encourage discussion, embellish harangues, are employed, and are of service in the duties of the professor, and the affairs of civil life. Nay, we openly express and declare that the philosophy we offer will not be very useful in such respects. It is not obvious, or to be understood in a cursory view, nor does it flatter the mind in its preconceived notions, nor will it descend to the level of the generality of mankind unless by its advantages and effects.

Let there exist, then (and may it be of advantage to both), two sources, and two distributions of learning, and in like manner two tribes, and as it were kindred families of contemplators or philosophers, without any hostility or alienation between them; but rather allied and united by mutual assistance. Let there be, in short, one method of cultivating the sciences, and another in discovering them. And as for those who prefer and more readily receive the former, on account of their haste or from motives arising from their ordinary life, or because they are unable from weakness of mind to comprehend and embrace the other (which must necessarily be the

case with by far the greater number), let us wish that they may prosper as they desire in their undertaking, and attain what they pursue. But if any individual desire, and is anxious not merely to adhere to, and make use of present discoveries, but to penetrate still further, and not to overcome his adversaries in disputes, but nature by labor, not in short to give elegant and specious opinions, but to know to a certainty and demonstration, let him, as a true son of science (if such be his wish), join with us; that when he has left the antechambers of nature trodden by the multitude, an entrance may at last be discovered to her inner apartments. And in order to be better understood, and to render our meaning more familiar by assigning determinate names, we have accustomed ourselves to call the one method the anticipation of the mind, and the other the interpretation of nature.

We have still one request left. We have at least reflected and taken pains, in order to render our propositions not only true, but of easy and familiar access to men's minds, however wonderfully prepossessed and limited. Yet it is but just that we should obtain this favor from mankind (especially in so great a restoration of learning and the sciences), that whosoever may be desirous of forming any determination upon an opinion of this our work either from his own perceptions, or the crowd of authorities, or the forms of demonstrations, he will not expect to be able to do so in a cursory manner, and whilst attending to other matters; but in order to have a thorough knowledge of the subject, will himself, by degrees, attempt the course which we describe and maintain; will be accustomed to the subtlety of things which is manifested by experience; and will correct the depraved and deeply-rooted habits of his mind by a seasonable, and, as it were, just hesitation: and then, finally (if he will), use his judgment when he has begun to be master of himself.

ON THE INTERPRETATION OF NATURE, OR THE REIGN OF MAN *

Man acts, then, upon natural bodies (besides merely bringing them together or removing them) by seven principal methods: I. By the exclusion of all that impedes and disturbs; II. by compression, extension, agitation, and the like; III. by heat and cold; IV. by detention

* Part II, Conclusion of the *Novum Organum*.

in a suitable place; V. by checking or directing motion; VI. by peculiar harmonies; VII. by a seasonable and proper alternation, series, and succession of all these, or, at least, of some of them.

I. With regard to the first—common air, which is always at hand, and forces its admission, as also the rays of the heavenly bodies, create much disturbance. Whatever, therefore, tends to exclude them may well be considered as generally useful. The substance and thickness of vessels in which bodies are placed when prepared for operations may be referred to this head. So also may the accurate methods of closing vessels by consolidation, or the *lutum sapientiæ* as the chemists call it. The exclusion of air by means of liquids at the extremity is also very useful, as when they pour oil on wine, or the juices of herbs, which by spreading itself upon the top like a cover, preserves them uninjured from the air. Powders, also, are serviceable, for although they contain air mixed up in them, yet they ward off the power of the mass of circumambient air, which is seen in the preservation of grapes and other fruits in sand or flour. Wax, honey, pitch, and other resinous bodies, are well used in order to make the exclusion more perfect, and to remove the air and celestial influence. We have sometimes made an experiment by placing a vessel or other bodies in quicksilver, the most dense of all substances capable of being poured round others. Grottoes and subterraneous caves are of great use in keeping off the effects of the sun, and the predatory action of air, and in the north of Germany are used for granaries. The depositing of bodies at the bottom of water may be also mentioned here; and I remember having heard of some bottles of wine being let down into a deep well in order to cool them, but left there by chance, carelessness, and forgetfulness, for several years, and then taken out; by which means the wine not only escaped becoming flat or dead, but was much more excellent in flavor, arising (as it appears) from a more complete mixture of its parts. But if the case require that bodies should be sunk to the bottom of water, as in rivers or the sea, and yet should not touch the water, nor be enclosed in sealed vessels, but surrounded only by air, it would be right to use that vessel which has been sometimes employed under water above ships that have sunk, in order to enable the divers to remain below and breathe occasionally by turns. It was of the following nature:—A hollow tub of metal was formed, and sunk

so as to have its bottom parallel with the surface of the water; it thus carried down with it to the bottom of the sea all the air contained in the tub. It stood upon three feet (like a tripod), being of rather less height than a man, so that, when the diver was in want of breath, he could put his head into the hollow of the tub, breathe, and then continue his work. We hear that some sort of boat or vessel has now been invented, capable of carrying men some distance under water. Any bodies, however, can easily be suspended under some such vessel as we have mentioned, which has occasioned our remarks upon the experiment.

Another advantage of the careful and hermetical closing of bodies is this—not only the admission of external air is prevented (of which we have treated), but the spirit of bodies also is prevented from making its escape, which is an internal operation. For anyone operating on natural bodies must be certain as to their quantity, and that nothing has evaporated or escaped, since profound alterations take place in bodies, when art prevents the loss or escape of any portion, whilst nature prevents their annihilation. With regard to this circumstance, a false idea has prevailed (which if true would make us despair of preserving quantity without diminution), namely, that the spirit of bodies, and air when rarefied by a great degree of heat, cannot be so kept in by being enclosed in any vessel as not to escape by the small pores. Men are led into this idea by the common experiments of a cup inverted over water, with a candle or piece of lighted paper in it, by which the water is drawn up, and of those cups which, when heated, draw up the flesh. For they think that in each experiment the rarefied air escapes, and that its quantity is therefore diminished, by which means the water or flesh rises by the motion of connection. This is, however, most incorrect. For the air is not diminished in quantity, but contracted in dimensions, nor does this motion of the rising of the water begin till the flame is extinguished, or the air cooled, so that physicians place cold sponges, moistened with water, on the cups, in order to increase their attraction. There is, therefore, no reason why men should fear much from the ready escape of air: for although it be true that the most solid bodies have their pores, yet neither air, nor spirit, readily suffers itself to be rarefied to such an extreme degree; just as water will not escape by a small chink.



II. With regard to the second of the seven above-mentioned methods, we must especially observe, that compression and similar violence have a most powerful effect either in producing locomotion, and other motions of the same nature, as may be observed in engines and projectiles, or in destroying the organic body, and those qualities, which consist entirely in motion (for all life, and every description of flame and ignition are destroyed by compression, which also injures and deranges every machine); or in destroying those qualities which consist in position and a coarse difference of parts, as in colors; for the color of a flower when whole, differs from that it presents when bruised, and the same may be observed of whole and powdered amber; or in tastes, for the taste of a pear before it is ripe, and of the same pear when bruised and softened, is different, since it becomes perceptibly more sweet. But such violence is of little avail in the more noble transformations and changes of homogeneous bodies, for they do not, by such means, acquire any constantly and permanently new state, but one that is transitory, and always struggling to return to its former habit and freedom. It would not, however, be useless to make some more diligent experiments with regard to this; whether, for instance, the condensation of a perfectly homogeneous body (such as air, water, oil, and the like) or their rarefaction, when effected by violence, can become permanent, fixed, and, as it were, so changed, as to become a nature. This might at first be tried by simple perseverance, and then by means of helps and harmonies. It might readily have been attempted (if we had but thought of it), when we condensed water (as was mentioned above), by hammering and compression, until it burst out. For we ought to have left the flattened globe untouched for some days, and then to have drawn off the water, in order to try whether it would have immediately occupied the same dimensions as it did before the condensation. If it had not been done so, either immediately, or soon afterwards, the condensation would have appeared to have been rendered constant; if not, it would have appeared that a restitution took place, and that the condensation had been transitory. Something of the same kind might have been tried with the glass eggs; the egg should have been sealed up suddenly and firmly, after a complete exhaustion of the air, and should have been allowed to remain so for some days, and it might then have been tried whether, on opening

the aperture, the air would be drawn in with a hissing noise, or whether as much water would be drawn into it when immersed, as would have been drawn into it at first, if it had not continued sealed. For it is probable (or, at least, worth making the experiment) that this might have happened, or might happen, because perseverance has a similar effect upon bodies which are a little less homogeneous. A stick bent together for some time does not rebound, which is not owing to any loss of quantity in the wood during the time, for the same would occur (after a larger time) in a plate of steel, which does not evaporate. If the experiment of simple perseverance should fail, the matter should not be given up, but other means should be employed. For it would be no small advantage, if bodies could be endued with fixed and constant natures by violence. Air could then be converted into water by condensation, with other similar effects; for man is more the master of violent motions than of any other means.

III. The third of our seven methods is referred to that great practical engine of nature as well as of art, cold and heat. Here, man's power limps, as it were, with one leg. For we possess the heat of fire, which is infinitely more powerful and intense than that of the sun (as it reaches us), and that of animals. But we want cold, except such as we can obtain in winter, in caverns, or by surrounding objects with snow and ice, which, perhaps, may be compared in degree with the noontide heat of the sun in tropical countries, increased by the reflection of mountains and walls. For this degree of heat and cold can be borne for a short period only by animals, yet it is nothing compared with the heat of a burning furnace, or the corresponding degree of cold. Everything with us has a tendency to become rarefied, dry, and wasted, and nothing to become condensed or soft, except by mixtures, and, as it were, spurious methods. Instances of cold, therefore, should be searched for most diligently, such as may be found by exposing bodies upon buildings in a hard frost, in subterraneous caverns, by surrounding bodies with snow and ice in deep places excavated for that purpose, by letting bodies down into wells, by burying bodies in quicksilver and metals, by immersing them in streams which petrify wood, by burying them in the earth (which the Chinese are reported to do with their china, masses of which, made for that purpose, are said

to remain in the ground for forty or fifty years, and to be transmitted to their heirs as a sort of artificial mine), and the like. The condensations which take place in nature, by means of cold, should also be investigated, that by learning their causes, they may be introduced into the arts; such as are observed in the exudation of marble and stones, in the dew upon the panes of glass in a room towards morning after a frosty night, in the formation and the gathering of vapors under the earth into water, whence spring fountains, and the like.

Besides the substances which are cold to the touch, there are others which have also the effect of cold, and condense; they appear, however, to act only upon the bodies of animals, and scarcely any further. Of these we have many instances, in medicines and plasters. Some condense the flesh and tangible parts, such as astringent and inspissating medicines, others the spirits, such as soporifics. There are two modes of condensing the spirits, by soporifics or provocatives to sleep; the one by calming the motion, the other by expelling the spirit. The violet, dried roses, lettuces, and other benign or mild remedies, by their friendly and gently cooling vapors, invite the spirits to unite, and restrain their violent and perturbed motion. Rosewater, for instance, applied to the nostrils in fainting fits, causes the resolved and relaxed spirits to recover themselves, and, as it were, cherishes them. But opiates, and the like, banish the spirits by their malignant and hostile quality. If they be applied, therefore, externally, the spirits immediately quit the part and no longer readily flow into it; but if they be taken internally, their vapor, mounting to the head, expels, in all directions, the spirits contained in the ventricles of the brain, and since these spirits retreat, but cannot escape, they consequently meet and are condensed, and are sometimes completely extinguished and suffocated; although the same opiates, when taken in moderation, by a secondary accident (the condensation which succeeds their union), strengthen the spirits, render them more robust, and check their useless and inflammatory motion, by which means they contribute not a little to the cure of diseases, and the prolongation of life.

The preparations of bodies, also, for the reception of cold should not be omitted, such as that water a little warmed is more easily frozen than that which is quite cold, and the like.

Moreover, since nature supplies cold so sparingly, we must act like

the apothecaries, who, when they cannot obtain any simple ingredient, take a succedaneum, or quid pro quo, as they term it, such as aloes for xylobalsamum, cassia for cinnamon. In the same manner we should look diligently about us, to ascertain whether there may be any substitutes for cold, that is to say, in what other manner condensation can be effected, which is the peculiar operation of cold. Such condensations appear hitherto to be of four kinds only. 1. By simple compression, which is of little avail towards permanent condensation, on account of the elasticity of substances, but may still however be of some assistance. 2. By the contraction of the coarser, after the escape or departure of the finer parts of a given body; as is exemplified in induration by fire, and the repeated heating and extinguishing of metals, and the like. 3. By the cohesion of the most solid homogeneous parts of a given body, which were previously separated, and mixed with others less solid, as in the return of sublimated mercury to its simple state, in which it occupies much less space than it did in powder, and the same may be observed of the cleansing of all metals from their dross. 4. By harmony or the application of substances which condense by some latent power. These harmonies are as yet but rarely observed, at which we cannot be surprised, since there is little to hope for from their investigation, unless the discovery of forms and conformation be attained. With regard to animal bodies, it is not to be questioned that there are many internal and external medicines which condense by harmony, as we have before observed, but this action is rare in inanimate bodies. Written accounts, as well as report, have certainly spoken of a tree in one of the Tercera or Canary Islands (for I do not exactly recollect which) that drips perpetually, so as to supply the inhabitants, in some degree, with water; and Paracelsus says that the herb called *ros solis* is filled with dew at noon, whilst the sun gives out its greatest heat, and all other herbs around it are dry. We treat both these accounts as fables; they would, however, if true, be of the most important service, and most worthy of examination. As to the honey-dew, resembling manna, which is found in May on the leaves of the oak, we are of opinion that it is not condensed by any harmony or peculiarity of the oak-leaf, but that whilst it falls equally upon other leaves it is retained and continues on those of the oak, because their texture is closer, and not so porous as that of most of the other leaves.

With regard to heat, man possesses abundant means and power; but his observation and inquiry are defective in some respects, and those of the greatest importance, notwithstanding the boasting of quacks. For the effects of intense heat are examined and observed, whilst those of a more gentle degree of heat, being of the most frequent occurrence in the paths of nature, are, on that very account, least known. We see, therefore, the furnaces, which are most esteemed, employed in increasing the spirits of bodies to a great extent, as in the strong acids, and some chemical oils; whilst the tangible parts are hardened, and, when the volatile part has escaped, become sometimes fixed; the homogeneous parts are separated, and the heterogeneous incorporated and agglomerated in a coarse lump; and (what is chiefly worthy of remark) the junction of compound bodies, and the more delicate conformations are destroyed and confounded. But the operation of a less violent heat should be tried and investigated, by which more delicate mixtures, and regular conformations may be produced and elicited, according to the example of nature, and in imitation of the effect of the sun, which we have alluded to in the aphorism on the instances of alliance. For the works of nature are carried on in much smaller portions, and in more delicate and varied positions than those of fire, as we now employ it. But man will then appear to have really augmented his power, when the works of nature can be imitated in species, perfected in power, and varied in quantity; to which should be added the acceleration in point of time. Rust, for instance, is the result of a long process, but *crocus martis* is obtained immediately; and the same may be observed of natural verdigris and ceruse. Crystal is formed slowly, whilst glass is blown immediately: stones increase slowly, whilst bricks are baked immediately, etc. In the mean time (with regard to our present subject) every different species of heat should, with its peculiar effects, be diligently collected and inquired into; that of the heavenly bodies, whether their rays be direct, reflected, or refracted, or condensed by a burning-glass; that of lightning, flame, and ignited charcoal; that of fire of different materials, either open or confined, straitened or overflowing, qualified by the different forms of the furnaces, excited by the bellows, or quiescent, removed to a greater or less distance, or passing through different media; moist heats, such as the *balneum Mariæ*, and the dunghill; the external and in-

ternal heat of animals; dry heats, such as the heat of ashes, lime, warm sand; in short, the nature of every kind of heat, and its degrees.

We should, however, particularly attend to the investigation and discovery of the effects and operations of heat, when made to approach and retire by degrees, regularly, periodically, and by proper intervals of space and time. For this systematical inequality is in truth the daughter of heaven and mother of generation, nor can any great result be expected from a vehement, precipitate, or desultory heat. For this is not only most evident in vegetables, but in the wombs of animals also there arises a great inequality of heat, from the motion, sleep, food, and passions of the female. The same inequality prevails in those subterraneous beds where metals and fossils are perpetually forming, which renders yet more remarkable the ignorance of some of the reformed alchemists, who imagined they could attain their object by the equable heat of lamps, or the like, burning uniformly. Let this suffice concerning the operation and effects of heat; nor is it time for us to investigate them thoroughly before the forms and conformations of bodies have been further examined and brought to light. When we have determined upon our models, we may seek, apply, and arrange our instruments.

IV. The fourth mode of action is by continuance, the very steward and almoner, as it were, of nature. We apply the term continuance to the abandonment of a body to itself for an observable time, guarded and protected in the mean while from all external force. For the internal motion then commences to betray and exert itself when the external and adventitious is removed. The effects of time, however, are far more delicate than those of fire. Wine, for instance, cannot be clarified by fire as it is by continuance. Nor are the ashes produced by combustion so fine as the particles dissolved or wasted by the lapse of ages. The incorporations and mixtures, which are hurried by fire, are very inferior to those obtained by continuance; and the various conformations assumed by bodies left to themselves, such as mouldiness, etc., are put a stop to by fire or a strong heat. It is not, in the mean time, unimportant to remark that there is a certain degree of violence in the motion of bodies entirely confined; for the confinement impedes the proper motion of the body. Continuance in an open vessel, therefore, is useful for separations, and in one hermetically sealed for mixtures, that in a vessel partly closed, but

admitting the air, for putrefaction. But instances of the operation and effect of continuance must be collected diligently from every quarter.

V. The direction of motion (which is the fifth method of action) is of no small use. We adopt this term, when speaking of a body which, meeting with another, either arrests, repels, allows, or directs its original motion. This is the case principally in the figure and position of vessels. An upright cone, for instance, promotes the condensation of vapor in alembics, but when reversed, as in inverted vessels, it assists the refining of sugar. Sometimes a curved form, or one alternately contracted and dilated, is required. Strainers may be ranged under this head, where the opposed body opens a way for one portion of another substance and impedes the rest. Nor is this process or any other direction of motion carried on externally only, but sometimes by one body within another. Thus, pebbles are thrown into water to collect the muddy particles, and syrups are refined by the white of an egg, which glues the grosser particles together so as to facilitate their removal. Telesius, indeed, rashly and ignorantly enough attributes the formation of animals to this cause, by means of the channels and folds of the womb. He ought to have observed a similar formation of the young in eggs which have no wrinkles or inequalities. One may observe a real result of this direction of motion in casting and modelling.

VI. The effects produced by harmony and aversion (which is the sixth method) are frequently buried in obscurity; for these occult and specific properties (as they are termed), the sympathies and antipathies, are for the most part but a corruption of philosophy. Nor can we form any great expectation of the discovery of the harmony which exists between natural objects, before that of their forms and simple conformations, for it is nothing more than the symmetry between these forms and conformations.

The greater and more universal species of harmony are not, however, so wholly obscure, and with them, therefore, we must commence. The first and principal distinction between them is this; that some bodies differ considerably in the abundance and rarity of their substance, but correspond in their conformation; others, on the contrary, correspond in the former and differ in the latter. Thus the chemists have well observed, that in their trial of first principles sulphur and

mercury, as it were, pervade the universe; their reasoning about salt, however, is absurd, and merely introduced to compromise earthy dry fixed bodies. In the other two, indeed, one of the most universal species of natural harmony manifests itself. Thus there is a correspondence between sulphur, oil, greasy exhalations, flame, and, perhaps, the substance of the stars. On the other hand, there is a like correspondence between mercury, water, aqueous vapor, air, and perhaps pure inter-sidereal ether. Yet do these two quaternions, or great natural tribes (each within its own limits), differ immensely in quantity and density of substance, whilst they generally agree in conformation, as is manifest in many instances. On the other hand, the metals agree in such quantity and density (especially when compared with vegetables, etc.), but differ in many respects in conformation. Animals and vegetables, in like manner, vary in their almost infinite modes of conformation, but range within very limited degrees of quantity and density of substance.

The next most general correspondence is that between individual bodies and those which supply them by way of menstruum or support. Inquiry, therefore, must be made as to the climate, soil, and depth at which each metal is generated, and the same of gems, whether produced in rocks or mines, also as to the soil in which particular trees, shrubs, and herbs, mostly grow and, as it were, delight; and as to the best species of manure, whether dung, chalk, sea sand, or ashes, etc., and their different propriety and advantage according to the variety of soils. So also the grafting and setting of trees and plants (as regards the readiness of grafting one particular species on another) depends very much upon harmony, and it would be amusing to try an experiment I have lately heard of, in grafting forest trees (garden trees alone having hitherto been adopted), by which means the leaves and fruit are enlarged, and the trees produce more shade. The specific food of animals again should be observed, as well as that which cannot be used. Thus the carnivorous cannot be fed on herbs, for which reason the order of *feuilletans*, the experiment having been made, has nearly vanished; human nature being incapable of supporting their regimen, although the human will has more power over the bodily frame than that of other animals. The different kinds of putrefaction from which animals are generated should be noted.

The harmony of principal bodies with those subordinate to them

(such indeed may be deemed those we have alluded to above) are sufficiently manifest, to which may be added those that exist between different bodies and their objects, and, since these latter are more apparent, they may throw great light when well observed and diligently examined upon those which are more latent.

The more internal harmony and aversion, or friendship and enmity (for superstition and folly have rendered the terms of sympathy and antipathy almost disgusting) have been either falsely assigned, or mixed with fable, or most rarely discovered from neglect. For if one were to allege that there is an enmity between the vine and the cabbage, because they will not come up well sown together, there is a sufficient reason for it in the succulent and absorbent nature of each plant, so that the one defrauds the other. Again, if one were to say that there is a harmony and friendship between the corn and the corn-flower, or the wild poppy, because the latter seldom grow anywhere but in cultivated soils, he ought rather to say, there is an enmity between them, for the poppy and the corn-flower are produced and created by those juices which the corn has left and rejected, so that the sowing of the corn prepares the ground for their production. And there are a vast number of similar false assertions. As for fables, they must be totally exterminated. There remains, then, but a scanty supply of such species of harmony as has borne the test of experiment, such as that between the magnet and iron, gold and quicksilver, and the like. In chemical experiments on metals, however, there are some others worthy of notice, but the greatest abundance (where the whole are so few in numbers) is discovered in certain medicines, which, from their occult and specific qualities (as they are termed), affect particular limbs, humors, diseases, or constitutions. Nor should we omit the harmony between the motion and phenomena of the moon, and their effects on lower bodies, which may be brought together by an accurate and honest selection from the experiments of agriculture, navigation, and medicine, or of other sciences. By as much as these general instances, however, of more latent harmony, are rare, with so much the more diligence are they to be inquired after, through tradition, and faithful and honest reports, but without rashness and credulity, with an anxious and, as it were, hesitating degree of reliance. There remains one species of harmony which, though simple in its mode of action, is yet most

valuable in its use, and must by no means be omitted, but rather diligently investigated. It is the ready or difficult coition or union of bodies in composition, or simple juxtaposition. For some bodies readily and willingly mix, and are incorporated, others tardily and perversely; thus powders mix best with water, chalk, and ashes with oils, and the like. Nor are these instances of readiness and aversion to mixture to be alone collected, but others, also, of the collocation, distribution, and digestion of the parts when mingled, and the predominance after the mixture is complete.

VII. Lastly, there remains the seventh, and last of the seven, modes of action; namely that by the alternation and interchange of the other six; but of this, it will not be the right time to offer any examples, until some deeper investigation shall have taken place of each of the others. The series, or chain of this alternation, in its mode of application to separate effects, is no less powerful in its operation, than difficult to be traced. But men are possessed with the most extreme impatience, both of such inquiries, and their practical application, although it be the clue of the labyrinth in all greater works.

But it must be noted, that in this our organ, we treat of logic, and not of philosophy. Seeing, however, that our logic instructs and informs the understanding, in order that it may not, with the small hooks, as it were, of the mind, catch at, and grasp mere abstractions, but rather actually penetrate nature, and discover the properties and effects of bodies, and the determinate laws of their substance (so that this science of ours springs from the nature of things, as well as from that of the mind); it is not to be wondered at, if it have been continually interspersed and illustrated with natural observations and experiments, as instances of our method. The prerogative instances are, as appears from what has preceded, twenty-seven in number, and are termed: solitary instances, migrating instances, conspicuous instances, clandestine instances, constitutive instances, similar instances, singular instances, deviating instances, bordering instances, instances of power, accompanying and hostile instances, subjunctive instances, instances of alliance, instances of the cross, instances of divorce, instances of the gate, citing instances, instances of the road, supplementary instances, lancing instances, instances of the rod, instances of the course, doses of nature, wrest-

ling instances, suggesting instances, generally useful instances, and magical instances. The advantage, by which these instances excel the more ordinary, regards specifically either theory or practice, or both. With regard to theory, they assist either the senses or the understanding; the senses, as in the five instances of the lamp; the understanding, either by expediting the exclusive mode of arriving at the form, as in solitary instances, or by confining, and more immediately indicating the affirmative, as in the migrating, conspicuous, accompanying, and subjunctive instances; or by elevating the understanding, and leading it to general and common natures, and that either immediately, as in the clandestine and singular instances, and those of alliance; or very nearly so, as in the constitutive; or still less so, as in the similar instances; or by correcting the understanding of its habits, as in the deviating instances; or by leading to the grand form or fabric of the universe, as in the bordering instances; or by guarding it from false forms and causes, as in those of the cross and of divorce. With regard to practice, they either point it out, or measure, or elevate it. They point it out, either by showing where we must commence in order not to repeat the labors of others, as in the instances of power; or by inducing us to aspire to that which may be possible, as in the suggesting instances; the four mathematical instances measure it. The generally useful and the magical elevate it.

Again, out of these twenty-seven instances, some must be collected immediately, without waiting for a particular investigation of properties. Such are the similar, singular, deviating, and bordering instances, those of power, and of the gate, and suggesting, generally useful, and magical instances; for these either assist and cure the understanding and senses, or furnish our general practice. The remainder are to be collected when we furnish our synoptical tables for the work of the interpreter, upon any particular nature; for these instances, honored and gifted with such prerogatives, are like the soul amid the vulgar crowd of instances, and (as we from the first observed) a few of them are worth a multitude of the others. When, therefore, we are forming our tables they must be searched out with the greatest zeal, and placed in the table. And, since mention must be made of them in what follows, a treatise upon their nature has necessarily been prefixed. We must next, however, proceed to the supports and corrections of induction, and thence to concretes, the

latent process, and latent conformations, and the other matters, which we have enumerated in their order in the twenty-first aphorism, in order that, like good and faithful guardians, we may yield up their fortune to mankind upon the emancipation and majority of their understanding; from which must necessarily follow an improvement of their estate, and an increase of their power over nature. For man, by the fall, lost at once his state of innocence, and his empire over creation, both of which can be partially recovered even in this life, the first by religion and faith, the second by the arts and sciences. For creation did not become entirely and utterly rebellious by the curse, but in consequence of the Divine decree, "in the sweat of thy brow shalt thou eat bread," she is compelled by our labors (not assuredly by our disputes or magical ceremonies), at length, to afford mankind in some degree his bread, that is to say, to supply man's daily wants.

II

NICOLAUS COPERNICUS

1473-1543

One of the first and most striking contributions to modern science was the substitution of the Copernican for the Ptolemaic conception of the universe.

Nicolaus Copernicus was born in the Prussian village of Thorn, located on the Vistula River, February 19, 1473. Although destined for the Church, he became interested in medicine, which he studied at the University of Cracow. Later, he turned to mathematics and continued his studies at the Universities of Vienna, Bologna, Padua, Ferrara, and Rome. Although he settled down as canon at Frauenberg, Poland, and gratuitously practised medicine in conjunction with his ecclesiastical duties, he found considerable time for other intellectual pursuits. Reading widely in the Greek philosophers, he came across a statement that the earth moved in its own orbit. This idea deeply appealed to him. "Occasioned by this," he wrote, "I also began to think of a motion of the earth, and although the idea seemed absurd, still, as others before me had been permitted to assume certain circles in order to explain the motions of the stars, I believed it would be readily permitted me to try whether on the assumption of some motion of the earth better explanations of the revolutions of the heavenly bodies might not be found. And thus I have, assuming the motions which I in the following work attribute to the earth, after long and careful investigation, finally found that when the motions of the other planets are referred to the circulation of the earth and are computed for the revolution of each star, not only do the phenomena necessarily follow therefrom, but the order and magnitude of the stars and all their orbs and the heaven itself are so connected that in no part can anything be transposed without confusion to the rest and to the whole universe."

In 1530 he issued a "Commentariolus" which outlined his theory,

but his prudence prompted him to withhold the publication of his great work, "*De Orbium Caelestium Revolutionibus*," until 1543. In May of that year the first printed copy was laid on his death-bed.

THE NEW IDEA OF THE UNIVERSE *

I can well believe, most holy father, that certain people, when they hear of my attributing motion to the earth in these books of mine, will at once declare that such an opinion ought to be rejected. Now, my own theories do not please me so much as not to consider what others may judge of them. Accordingly, when I began to reflect upon what those persons who accept the stability of the earth, as confirmed by the opinion of many centuries, would say when I claimed that the earth moves, I hesitated for a long time as to whether I should publish that which I have written to demonstrate its motion, or whether it would not be better to follow the example of the Pythagoreans, who used to hand down the secrets of philosophy to their relatives and friends only in oral form. As I well considered all this, I was almost impelled to put the finished work wholly aside, through the scorn I had reason to anticipate on account of the newness and apparent contrariness of my theory to reason.

My friends, however, dissuaded me from such a course and admonished me that I ought to publish my book, which had lain concealed in my possession not only nine years, but already into four times the ninth year. Not a few other distinguished and very learned men asked me to do the same thing, and told me that I ought not, on account of my anxiety, to delay any longer in consecrating my work to the general service of mathematicians.

But your holiness will perhaps not so much wonder that I have dared to bring the results of my night labors to the light of day, after having taken so much care in elaborating them, but is waiting instead to hear how it entered my mind to imagine that the earth moved, contrary to the accepted opinion of mathematicians—nay, almost contrary to ordinary human understanding. Therefore I will not conceal from your holiness that what moved me to consider another way of reckoning the motions of the heavenly bodies was

* Selections from the Introduction to *De Orbium Caelestium Revolutionibus*.

nothing else than the fact that the mathematicians do not agree with one another in their investigations. In the first place, they are so uncertain about the motions of the sun and moon that they cannot find out the length of a full year. In the second place, they apply neither the same laws of cause and effect, in determining the motions of the sun and moon and of the five planets, nor the same proofs. Some employ only concentric circles, others use eccentric and epicyclic ones, with which, however, they do not fully attain the desired end. They could not even discover nor compute the main thing—namely, the form of the universe and the symmetry of its parts. It was with them as if some should, from different places, take hands, feet, head, and other parts of the body, which, although very beautiful, were not drawn in their proper relations, and, without making them in any way correspond, should construct a monster instead of a human being.

Accordingly, when I had long reflected, on this uncertainty of mathematical tradition, I took the trouble to read again the books of all the philosophers I could get hold of, to see if some one of them had not once believed that there were other motions of the heavenly bodies. First I found in Cicero that Niceties had believed in the motion of the earth. Afterwards I found in Plutarch, likewise, that some others had held the same opinion. This induced me also to begin to consider the movability of the earth, and, although the theory appeared contrary to reason, I did so because I knew that others before me had been allowed to assume rotary movements at will, in order to explain the phenomena of these celestial bodies. I was of the opinion that I, too, might be permitted to see whether, by pre-supposing motion in the earth, more reliable conclusions than hitherto reached could not be discovered for the rotary motions of the spheres. And thus, acting on the hypothesis of the motion which, in the following book, I ascribe to the earth, and by long and continued observations, I have finally discovered that if the motion of the other planets be carried over to the relation of the earth and this is made the basis for the rotation of every star, not only will the phenomena of the planets be explained thereby, but also the laws and the size of the stars; all their spheres and the heavens themselves will appear so harmoniously connected that nothing could be changed in any part of them without confusion in the remaining parts and in the whole universe.

THAT THE UNIVERSE IS SPHERICAL

First we must remark that the universe is spherical in form, partly because this form being a perfect whole requiring no joints, is the most complete of all, partly because it makes the most capacious form, which is best suited to contain and preserve everything; or again because all the constituent parts of the universe, that is the sun, moon, and the planets appear in this form; or because everything strives to attain this form, as appears in the case of drops of water and other fluid bodies if they attempt to define themselves. So no one will doubt that this form belongs to the heavenly bodies.

THAT THE EARTH IS ALSO SPHERICAL

That the earth is also spherical is therefore beyond question, because it presses from all sides upon its center. Although by reason of the elevations of the mountains and the depressions of the valleys a perfect circle cannot be understood, yet this does not affect the general spherical nature of the earth. This appears in the following manner. To those who journey towards the North the north pole of the daily revolution of the heavenly sphere seems gradually to rise, while the opposite seems to sink. Most of the stars in the region of the Bear seem not to set, while some of the southern stars seem not to rise at all. So Italy does not see Canopes which is visible to the Egyptians. And Italy sees the outermost star of the Stream, which our region of a colder zone does not know. On the other hand to those who go towards the South the others seem to rise and those to sink which are high in our region. Moreover, the inclination of the Poles to the diameter of the earth bears always the same relation, which could happen only in the case of a sphere. So it is evident that the earth is included between the two poles, and is therefore spherical in form. Let us add that the inhabitants of the East do not observe the eclipse of the sun or of the moon which occurs in the evening, and the inhabitants of the West those which occur in the morning, while those who dwell between see those later and these earlier. That the water also has the same form can be observed from ships, in that the land which cannot be seen from the deck, is visible from the mast-tree. And conversely if a light be placed at the mast-head it seems

to those who remain on the shores gradually to sink and at last still sinking to disappear. It is clear that the water also according to its nature continually presses like the earth downward, and does not rise above its banks higher than its convexity permits. So the land extends above the ocean as much as the land happens to be higher.

WHETHER THE EARTH HAS A CIRCULAR MOTION, AND CONCERNING THE LOCATION OF THE EARTH

As it has been already shown that the earth has the form of a sphere, we must consider whether a movement also coincides with this form, and what place the earth holds in the universe. Without this there will be no secure results to be obtained in regard to the heavenly phenomena. The great majority of authors of course agree that the earth stands still in the center of the universe, and consider it inconceivable and ridiculous to suppose the opposite. But if the matter is carefully weighed it will be seen that the question is not yet settled and therefore by no means to be regarded lightly. Every change of place which is observed is due, namely, to a movement of the observed object or of the observer, or to movements of both, naturally in different directions, for if the observed object and the observer move in the same manner and in the same direction no movement will be seen. Now it is from the earth that the revolution of the heavens is observed and it is produced for our eyes. Therefore if the earth undergoes no movement this movement must take place in everything outside of the earth, but in the opposite direction than if everything on the earth moved, and of this kind is the daily revolution. So this appears to affect the whole universe, that is, everything outside the earth with the single exception of the earth itself. If, however, one should admit that this movement was not peculiar to the heavens, but that the earth revolved from west to east, and if this was carefully considered in regard to the apparent rising and setting of the sun, the moon and the stars, it would be discovered that this was the real situation. Since the sky, which contains and shelters all things, is the common seat of all things, it is not easy to understand why motion should not be ascribed rather to the thing contained than to the containing, to the located rather than to the location. From this supposition follows another question of no less importance, concerning the place of the

earth, although it has been accepted and believed by almost all, that the earth occupies the middle of the universe. But if one should suppose that the earth is not at the center of the universe, that, however, the distance between the two is not great enough to be measured on the orbits of the fixed stars, but would be noticeable and perceptible on the orbit of the sun or of the planets: and if one was further of the opinion that the movements of the planets appeared to be irregular as if they were governed by a center other than the earth, then such an one could perhaps have given the true reasons for the apparently irregular movement. For since the planets appear now nearer and now farther from the earth, this shows necessarily that the center of their revolutions is not the center of the earth: although it does not settle whether the earth increases and decreases the distance from them or they their distance from the earth.

REFUTATION OF THE ARGUMENT OF THE ANCIENTS THAT THE
EARTH REMAINS STILL IN THE MIDDLE OF THE UNIVERSE,
AS IF IT WERE ITS CENTER

From this and similar reasons it is supposed that the earth rests at the center of the universe and that there is no doubt of the fact. But if one believed that the earth revolved, he would certainly be of the opinion that this movement was natural and not arbitrary. For whatever is in accord with nature produces results which are the opposite of those produced by force. Things upon which force or an outside power has acted, must be injured and cannot long endure: what happens by nature, however, preserves itself well and exists in the best condition. So Ptolemy feared without good reason that the earth and all earthly objects subject to the revolution would be destroyed by the act of nature, since this latter is opposed to artificial acts, or to what is produced by the human spirit. But why did not he fear the same, and in a much higher degree, of the universe, whose motion must be as much more rapid as the heavens are greater than the earth? Or has the heaven become so immense because it has been driven outward from the center by the inconceivable power of the revolution; while if it stood still, on the contrary, it would collapse and fall together? But surely if this is the case the extent of the heavens would increase infinitely. For the more it is driven higher by the outward force of the

movement, so much the more rapid will the movement become, because of the ever increasing circle which must be traversed in 24 hours ; and conversely if the movement grows the immensity of the heavens grows. So the velocity would increase the size and the size would increase the velocity unendingly. According to the physical law that the endless cannot wear away nor in any way move, the heavens must necessarily stand still.

But it is said that beyond the sky no body, no place, no vacant space, in fact nothing at all exists ; then it is strange that some thing should be enclosed by nothing. But if the heaven is endless and is bounded only by the inner hollow, perhaps this establishes all the more clearly the fact that there is nothing outside the heavens, because everything is within it, but the heaven must then remain unmoved. The highest proof on which one supports the finite character of the universe is its movement. But whether the universe is endless or limited we will leave to the physiologues ; this remains sure for us that the earth enclosed between the poles, is bounded by a spherical surface. Why therefore should we not take the position of ascribing to a movement conformable to its nature and corresponding to its form, rather than suppose that the whole universe whose limits are not and cannot be known moves ? and why will we not recognize that the appearance of a daily revolution belongs to the heavens, but the actuality to the earth ; and that the relation is similar to that of which one says : "We run out of the harbor, the lands and cities retreat from us." Because if a ship sails along quietly, everything outside of it appears to those on board as if it moved with the motion of the boat, and the boatman thinks that the boat with all on board is standing still, this same thing may hold without doubt of the motion of the earth, and it may seem as if the whole universe revolved. What shall we say, however, of the clouds and other things floating, falling or raising in the air—except that not only does the earth move with the watery elements belonging with it, but also a large part of the atmosphere, and whatever else is in any way connected with the earth ; whether it is because the air immediately touching the earth has the same nature as the earth, or that the motion has become imparted to the atmosphere. A like astonishment must be felt if that highest region of the air be supposed to follow the heavenly motion, as shown by those suddenly appearing stars which the Greeks call comets or bearded stars, which

belong to that region and which rise and set like other stars. We may suppose that part of the atmosphere, because of its great distance from the earth, has become free from the earthly motion. So the atmosphere which lies close to the earth and all things floating in it would appear to remain still, unless driven here and there by the wind or some other outside force, which chance may bring into play; for how is the wind in the air different from the current in the sea? We must admit that the motion of things rising and falling in the air is in relation to the universe a double one, being always made up of a rectilinear and a circular movement. Since that which seeks of its own weight to fall is essentially earthly, so there is no doubt that these follow the same natural law as their whole; and it results from the same principle that those things which pertain to fire are forcibly driven on high. Earthly fire is nourished with earthly stuff, and it is said that the flame is only burning smoke. But the peculiarity of the fire consists in this that it expands whatever it seizes upon, and it carries this out so consistently that it can in no way and by no machinery be prevented from breaking its bonds and completing its work. The expanding motion, however, is directed from the center outward; therefore if any earthly material is ignited it moves upward. So to each single body belongs a single motion, and this is evinced preferably in a circular direction as long as the single body remains in its natural place and its entirety. In this position the movement is the circular movement which as far as the body itself is concerned is as if it did not occur. The rectilinear motion, however, seizes upon those bodies which have wandered or have been driven from their natural position or have been in any way disturbed. Nothing is so much opposed to the order and form of the world as the displacement of one of its parts. Rectilinear motion takes place only when objects are not properly related, and are not complete according to their nature because they have separated from their whole and have lost their unity. Moreover, objects which have been driven outward or away, leaving out of consideration the circular motion, do not obey a single, simple and regular motion, since they cannot be controlled simply by their lightness or by the force of their weight, and if in falling they have at first a slow movement the rapidity of the motion increases as they fall, while in the case of earthly fire which is forced upwards—and we have no means of knowing any other kind of fire—we will see that its motion

is slow as if its earthly origin thereby showed itself. The circular motion, on the other hand, is always regular, because it is not subject to an intermittent cause. Those other objects, however, would cease to be either light or heavy in respect to their natural movement if they reached their own place, and thus they would fit into that movement. Therefore if the circular movement is to be ascribed to the universe as a whole and the rectilinear to the parts, we might say that the revolution is to the straight line as the natural state is to sickness. That Aristotle divided motion into three sorts, that from the center out, that inward toward the center, and that around about the center, appears to be merely a logical convenience, just as we distinguish point, line and surface, although one cannot exist without the others, and none of them are found apart from bodies. This fact is also to be considered, that the condition of immovability is held to be nobler and more divine than that of change and inconstancy, which latter therefore should be ascribed rather to the earth than to the universe, and I would add also that it seems inconsistent to attribute motion to the containing and locating element rather than to the contained and located object, which the earth is. Finally since the planets plainly are at one time nearer and at another time farther from the earth, it would follow, on the theory that the universe revolves, that the movement of the one and same body which is known to take place about a center, that is the center of the earth, must also be directed toward the center from without and from the center outward. The movement about the center must therefore be made more general, and it suffices if that single movement be about its own center. So it appears from all these considerations that the movement of the earth is more probable than its fixity, especially in regard to the daily revolution, which is most peculiar to the earth.

III

JOHANN KEPLER

1571-1630

Tycho Brahe (1546-1601), nobleman of Denmark, studied law at the University of Copenhagen and became attracted to astronomical studies by the occurrence of a predicted eclipse. Constructing his own instruments, he made observations of the stars at Augsburg and Wittenberg, and in 1576 established the first observatory at Huen, where he continued his work for twenty years. Banished from Germany, he was invited by Emperor Rudolph to Prague, where he began his compilation of the Rudolphin Tables which listed many of his observations on the locations of the planets. Hearing of Kepler's interest in astronomy, he secured the young German's assistance and assigned to him the study of the planet Mars, which study Kepler continued after Tycho Brahe's death in 1601.

Johann Kepler, the son of an innkeeper, was born December 27, 1571, in Württemberg and sent to a local school, from which he was removed when he was nine years old because of his father's impoverishment. After three years of work in the tavern, he was sent to a monastic school and thence to the University of Tübingen. Although he was very frail in physique, he was a good student and attained high scholarly standing. Becoming interested in the Copernican theory, in 1599 he was invited by Tycho Brahe to become his assistant at Prague.

Kepler found his master's tables sufficiently accurate in his efforts to discover some recognizable motion of the planet Mars which would account for its apparent positions. In the course of this work he corrected some of the Ptolemaic ideas which Copernicus had not completely abandoned. The latter retained the epicycle motion of the planets within their larger revolutions in cycles. In comparing this theory with his tables, Kepler found that it would not satisfactorily account for the positions of Mars; and he was therefore led to the long studies and mathematical computations which finally resulted in

his discovery of the orbit of Mars, and to the establishment of the first two of his three famous laws: "1. the planet describes an ellipse, the sun being in one focus; 2. the straight line joining the planet to the sun sweeps out equal areas in equal intervals of time." (Sedgwick and Tyler, pp. 211-213). He published these laws in 1609 in his "*Commentaries on the Motions of Mars*."

In 1611, when his patron, Emperor Rudolph, was compelled to abdicate, Kepler was left penniless, but he moved to Linz where he was appointed to a professorship. In 1619 he published his "*Harmony of the World*," which contained his third law: "The squares of the times of revolution of any two planets (including the earth) about the sun are proportional to the cubes of their mean distances from the sun." (Sedgwick and Tyler, p. 213). This was the triumph about which he wrote in the year of its discovery, 1618: "What I prophesied twenty-two years ago, as soon as I found the heavenly orbits were of the same number as the five (regular) solids, what I fully believed long before I had seen Ptolemy's *Harmonies*, what I promised my friends in the name of this book, which I christened before I was sixteen years old, I urged as an end to be sought, that for which I joined Tycho Brahe, for which I settled at Prague, for which I have spent most of my life at astronomical calculations—at last I have brought to light, and seen to be true beyond my fondest hopes. It is not eighteen months since I saw the first ray of light, three months since the unclouded sun-glorious sight! burst upon me. Let nothing confine me: I will indulge my sacred ecstasy. I will triumph over mankind by the honest confession that I have stolen the golden vases of the Egyptians to raise a tabernacle for my God far away from the lands of Egypt. If you forgive me, I rejoice; if you are angry, I cannot help it. The book is written; the die is cast. Let it be read now or by posterity, I care not which. It may well wait a century for a reader, as God had waited six thousand years for an observer." Kepler died at Ratisbon, November 15, 1630.

ON THE PRINCIPLES OF ASTRONOMY *

What is astronomy? It is the science of treating of the causes of those celestial appearances which we who live on the earth observe and which mark the changes of times and seasons; by the studying of

* From *The Epitome of Astronomy*

which we are able to predict for the future the face of the heavens, that is, the stellar phenomena, and to assign fixed dates for those which have occurred in the past.

Why is it called astronomy? From the law (*νομος*) or governance of the stars (*ἀστροα*), that is, of the motions in which the stars move, just as economy is named from the law of domestic affairs (*οἰκονομία*) and paedonomy (*παιδονομία*) from the ruling of youths.

What is the relation of this science to the other sciences? 1) It is a branch of physics because it investigates the causes of natural objects and events, and because among its subjects are the motions of the heavenly bodies, and because it has the same end as physics, to inquire into the conformation of the world and its parts.

2) Astronomy is the soul of geography and hydrography, for the various appearances of the sky in various districts and regions of the earth and sea are known only by astronomy.

3) Chronology is dependent upon it, because the movements of the heavenly bodies prescribe seasons and years and date the histories.

4) Meteorology is also its subordinate, for the stars move and influence this sublunary nature and even men themselves.

5) It includes a large part of optics, because it has a subject in common with that; that is, the light of the heavenly bodies, and because it corrects many errors of sight in regard to the character of the earth and its motions.

6) It is, however, subordinate to the general subject of mathematics and uses arithmetic and geometry as its two wings, studying the extent and form of the bodies and motions of the universe and computing the periods, by these means expediting its demonstrations and reducing them to use and practical value.

How many, then, are the branches of astronomical study? The departments of the study of astronomy are five; historical, in the matter of observations, optical as to the hypothesis, physical as to the causes of the hypotheses, arithmetical as to the tables and calculations, mechanical as to its instruments.

Since we must begin with appearances, explain how the world seems to be made up. The world is commonly thought, accepting the testimony of the eyes, to be an immense structure consisting of two parts, the earth and the sky.

What do men imagine concerning the figure of the earth? The earth seems to be a broad plane extending in a circle in every direction around the spectator. And from this appearance of a plane bounded by a great circle the appellation, *orbis terrarum*, the circle of the earth, has arisen, and has been taken over by the Scripture and among other nations.

What do men imagine to be the center of the earth? Each nation, unless it has become familiar with the notion of the circle, thinks by the instinct of nature and the error of vision that its country is in the center or middle of this plane circle. So the common people among the Jews believe still that Jerusalem, the earliest home of their race, is situated at the center of the world.

What do men think about the waters? Since men proceeding as far as possible in any direction finally came upon the ocean, some have thought that the earth is like a disc swimming in the waters, and that the waters are held up by the lower part of the sky, whence poets have called the ocean, the father of all things. Others believe that a strip of land surrounds the ocean which keeps the water from flowing away, and these suppose there is land under the water, saying that the water is held up by the earth. Besides these there are still others who, since the ocean seems higher than the land if it is looked at from the edge of the shore, believe that the earth is, as it were, sunk in the waters and supernaturally guarded by the omnipotence of God lest the waters rushing in from the deep should overwhelm it.

What do men imagine to be under both the land and the waters? There has been great discussion among men marveling concerning the foundation which could bear up the great mass of the earth so that it should remain for so many centuries firm and immovable and should not sink; and Heraclitus among the early philosophers, and Lactantius among the ecclesiastics said that it reached down to the lowest root of things.

How about the other part of the world, the sky and its extent? Men have thought that the sky was not much larger than the earth, and indeed was connected with the earth and the ocean at the circumference of the circle, so that it bounded the earth; and that anyone going that far, if it could be done, would run up against the sky, blocking further progress. With this idea of men the Scriptures also agreed.

So also the poets said that Mt. Atlas, a lofty mountain on the

farthest shore of Africa, bore up the sky on his shoulders, and Homer placed the Aethiopeans at the extremities of the rising and setting sun, thinking that because of the contiguity of the earth and sky there, the sun was so close to them that it burned their skin.

What form do they ascribe to the sky? The eyes ascribe to the sky the shape of a tent, extending over our heads and beyond the sun, moon and stars, or rather the shape of an arch overspanning the terrestrial plane, with a long curve, so that the part of the sky just over the head of the spectator is much nearer to him than the part that touches the mountains.

What have men conceived in regard to the motion of the sky? Whether the sky moves or stands still is not apparent to the sight because the tenuity of its substance escapes the eyes, unless indeed those things appear to stand still in which the eye can perceive no variation. But the changing positions of the sun, moon and stars in relation to the ends of the earth was apparent to the eyes. For the sun seems to emerge from an opening between the sky and the immovable mountains and ocean, as if coming out of a chamber, and having traversed the vault of the sky seems to sink again in the opposite region; so also the moon, and the planets, and the whole host of stars proceed as if strictly marshalled and drawn up in line, first one and then the other marching along, each in his order and place.

And so, since the ocean lies beyond the extreme lands, the mass of men have thought that the sun plunges into the ocean and is extinguished, and from the opposite region a new sun issues forth daily from the ocean. The poets have used this figure in their creations. But, indeed, there have been even philosophers who have declared that on the farthest shores of Lusitania could be heard the roar of the ocean extinguishing the flames of the sun, as Strabo recounts.

I understand the forms of the sky and the earth and the atmosphere surrounding the earth, also the place of the earth in the universe; now I would ask what causes the stars to seem to rise daily from the one part of the horizon and to sink in the opposite part; the motion of the sky or of the earth? The astronomy of Copernicus shows that our sight has led us astray in regard to this motion; for the stars do not actually come up from beyond the mountains and climb toward the zenith, but rather the mountains which surround us and which are a

part of the surface of the earth are revolved along with the whole globe about its axis from west to east and by this revolution the immovable stars of the east are disclosed to us one after the other, and those of the west are obscured, so the stars are not passing over us, but the vertical point is moving through the fixed stars.

You say that by this marvelous hypothesis may be explained satisfactorily all the phenomena of the first motion and the spherical theory. Just so, and that is the scope of this section, to demonstrate in fact what has been suggested in words.

How do you expect to be able to prove this absurd hypothesis, and by what arguments? It is possible to demonstrate that this first motion results from the revolution of the earth about its axis, while the heavenly bodies are at rest (as far as this first motion is concerned), by seven kinds of arguments: 1) from the subject of the motion; 2) from the velocity of the motion; 3) from the equableness of the motion; 4) from the cause of the motion, or the moving principle; 5) from the motive instruments, that is, the axis and the poles; 6) from the object of the first motion; and 7) from the indications or results.

Demonstrate it then from the subject of the motion. Nature does not seek difficult means when she can use simple ones. Now, by the rotation of the earth, a very small body, about its axis, toward the east, the same thing is accomplished as by the rotation of the immense universe about its axis toward the west. Just as it is more likely that a man's head turns in the auditorium than that the auditorium is turned about his head, so it is more credible that the earth is rotating from west to east, than that the rest of the machine of the universe is revolved from east to west, since in both cases the same thing results.

If the first motion is in the heavenly bodies, then they are subject to two motions, one common to the whole universe, the other particular to each sphere; but it is much more probable that the two motions should be distinct in regard to their subjects, so that the second set of motions, which is multifold, should belong to each sphere, and the first, which is single, should belong to the single body of the earth, and to it alone.

Why cannot the whole machinery of the universe be moved? The universe is either infinite or finite. Suppose it to be the former, according to the opinion of William Gilbert, who thinks that the

omnipotence of God is illustrated in this that the universe extends outward infinitely, so that the infinite power of the creator would be recognized from the infinite extent of the creation. Although this may be refuted by metaphysical arguments, no argument on either side can be drawn from astronomy, in which trust is placed rather in the evidence of the senses than in abstract reasonings not dependent on observation. But supposing this universe to be infinite, Aristotle has shown that the whole universe should not be moved about in a revolution since it is the whole.

But let the universe be finite; then there is nothing outside the universe which would locate the universe but should remain quiet itself. Where there is nothing that rests there is no motion. For 1) motion is the separation of a movable thing from its place and its transfer to another place: 2) the motion of a machine about an axis and quiescent poles cannot be grasped by the mind where there is no thing in respect to which the poles remain still.



IV

GALILEO GALILEI

1564-1642

Galileo Galilei, born at Pisa, February 15, 1564, was the son of a mathematician who, seeing no future in that profession, had him educated for the practice of medicine. But when Galileo was about eighteen years of age, while observing a large lamp swinging in the Pisa cathedral, he noticed that, regardless of the length of the oscillation, the time did not vary. In spite of his father's discouragements, therefore, he became absorbed in mathematics and abandoned the study of medicine. Applying himself to the study of motion, he performed his famous experiment of letting bodies of different weights fall from the leaning tower of Pisa, proving that things of unequal weight, if heavier than the resistance of air, fall with the same speed. The doctrine of inertia which he deduced from this and similar experiments decisively answered the opponents of Copernicus; for the principle stated that bodies would continue to move in the same direction forever unless their course was disturbed or opposed by another force, and that the motion of bodies resulted from independent forces operating upon them. His treatise on the center of gravity in solids earned him a lectureship at the University of Pisa.

Meeting malignant opposition at Pisa, he secured the chair of mathematics at Padua (which he held from 1592 to 1610) and there continued his observations and experiments in physics and chemistry. He succeeded in making a crude thermometer in 1600. In 1609 he learned that Hans Lippershey, an optician of Middleburg, had succeeded in making a telescope. He thereupon made one of his own and improved it until it had a power of magnifying thirty-two times, enabling him to discover the mountainous surface of the moon, the moons of the planet Jupiter, the fact that Venus showed different

sides like the moon, and that many small stars made up the Milky Way.

In 1610 he left Padua for Florence, and by 1613 openly declared his acceptance of Copernican ideas. Immediately he was opposed by theologians, and after being given an opportunity to renounce his adherence to the new system of astronomy, was sentenced in 1616 not to hold, teach, or defend it. In 1623, when his friend Maffeo was made Pope Urban VIII, he wrote his dialogues on the system of the world. He had much difficulty in getting them published and succeeded only when he assured the authorities that they were not heretical. It was quite evident, however, that the dialogues were slightly concealed arguments for the acceptance of the Copernican system and consequently in 1633 he was summoned before the Inquisition and compelled to renounce his heresy. In 1637, a few months after he had discovered the librations of the moon, he lost his sight. He died five years later, January 8, 1642.

THE COPERNICAN VERSUS THE PTOLEMAIC ASTRONOMIES *

Formerly I used frequently to visit the marvelous city of Venice and to meet there Signore Giovan Francesco Sagredo, a man of most distinguished ancestry and remarkable intelligence. Thither also came from Florence, Signore Filippo Salviati, whose least claim to renown was his noble blood and great wealth; a noble mind, that held no enjoyment of greater price than that of study and thought. With both of these men I often discussed these questions, in the presence of a Peripatetic philosopher, who apparently valued the acquisition of knowledge in no way in so high a degree, as he did the renown which his interpretations of Aristotle had gained for him.

Now that cruel death has robbed the cities of Venice and Florence of these two enlightened men in the bloom of their years, I have endeavored, as far as my weak powers may permit, to perpetuate their fame in these pages by making them the speakers in this dialogue. The valiant Peripatetic also shall not fail to appear; because of his over-weaning love for the commentary of Simplicius, it seemed permissible to omit his own name and let him pass under that of his favorite author. May the souls of these two great men accept this

* Translated from the *Dialogo dei due Massima Systemi del Mondo* (1632).

public testimony of my undying love; may the recollection of their eloquence aid me in setting down for posterity the spoken discussions.

SECOND DAY

SALVIATI: We departed yesterday so often and so far from the direct path of our discussion, that I can scarcely return to the right point and proceed without your help.

SAGREDO: I find it quite intelligible that you are somewhat at a loss, since you have had your head so full of both the things already brought forward and things still to be discussed. I, however, who as merely a listener have in mind only the things already discussed, may I hope set our investigation straight by a brief summary of what has been gone over. So, if my memory fails not, the chief result of our yesterday's conversation was that we tested thoroughly which of the two theories was the more probable and better grounded; that according to which the substance of the heavenly bodies is unproduced, indestructible, unchangeable, intangible, in brief not subject to any variation aside from change of location, and so presents a fifth element which is entirely distinct from our elementary, producible, destructible, changeable bodies; or the other view, according to which an incongruity between parts of the universe is rejected, our earth rather enjoys the same privileges as the rest of the constituent bodies of the universe, in a word, is a freely moving ball just as the moon, Jupiter, Venus, or any other planet. Finally we noticed the many similarities in particular between the earth and the moon, and of course with the moon more than any other planet because of the closer and more definite knowledge which we possess of it by reason of its less distance. Since we agreed that this second opinion possessed the greater probability, the logical consequence, it seems to me, is that we should investigate the question whether we should hold the world immovable, as has been formerly believed in general, or movable as some ancient philosophers believed and as some recent ones suppose: and if movable, how its movement could have been produced.

SALV.: Let us begin our discussion with the admission that whatever sort of motion may be ascribed the earth, we, as its inhabitants

and therefore partakers in the movement, would be unconscious of it, as if it did not occur, since we can only take into consideration earthly things. Therefore it is necessary that this movement should seem to belong to all the other bodies and visible objects in common which, separated from the earth, have no share in its movement. The correct method of determining whether movement is to be attributed to the earth, and what movement, is that one should inquire and observe whether an apparent movement can be ascribed to the bodies outside of the earth, which belongs to all of them in the same degree. So a movement which, for example, can be supposed of the moon, and not of Venus or Jupiter or other stars, cannot be peculiar to the earth. Now there is such a general movement governing all other objects, namely that which the sun, moon, planets, fixed stars, in a word the whole universe with the single exception of the earth, seems to follow from east to west within the space of twenty-four hours. This, at least at first glance, may be just as well attributed to the earth alone, as to the rest of the entire universe except the earth.

SAGR.: I understand clearly that your suggestion is correct. An objection, however, forces itself upon me that I cannot solve. That is, since Copernicus ascribes to the earth a further movement aside from the daily one, according to the above mentioned principle this should be apparently un-noticeable on the earth, but should be visible in the rest of the universe. I come then to the conclusion that either he plainly erred when he ascribed to the earth a movement to which no counterpart is apparent in the firmament, or else such a movement exists, and then Ptolemaus is guilty of a second error in that he did not refute with arguments this movement as well as that daily rotation.

SALV.: Your objection is very just. If we take up this other movement, you shall see how much superior in intelligence was Copernicus to Ptolemaus, in that he saw what this one did not, namely how wonderfully this second motion is reflected in the rest of the heavenly bodies. For the present, however, we will leave this aside and return to our first consideration. Proceeding from the most general suppositions, I will present the arguments which seem to favor the motion of the earth, in order then to hear the opposing arguments of Signore Simplicio. First, then, when we consider the immense circumference of the stellar sphere in comparison with the

smallness of the earth, which is contained in that several million times, and therefore regard the velocity of motion which would be necessary for an entire revolution in the course of a day and night, I am unable to understand how any one could hold it more reasonable and credible that it is this whole stellar sphere that moves and that the earth remains still.

SAGR.: Even if universal phenomena which depend upon these movements could be explained as readily by the one hypothesis as by the other, yet by the first general impression I would regard as more unreasonable the view that the whole universe moves; just as if any one should climb to the top of your dome for the purpose of getting a view of the city and its environs and then should demand that the whole region be made to move around him to save him the trouble of turning his head. In any event, there would have to be great advantages connected with this theory, which were lacking in the other, in order that such an absurdity should be balanced and outweighed and should appear more credible than the opposite opinion. But Aristotle, Ptolemaus, and Signore Simplicio must find such advantages in their theory, and I should be glad if we might hear these advantages if they exist, or if they do not, that some one would explain to me why they do not and cannot exist.

SALV.: If, in spite of every sort of investigation, I am able to find no such differences, I believe I have thereby discovered that such difference does not exist. So in my opinion it is useless to pursue this further: rather let us proceed. Motion is only so far motion and acts as such, if it stands in relation to things which lack motion. In relation to things that are all in the same degree affected by it, it is as much without effect as if it did not take place. The wares with which a ship is loaded move, when they depart from Venice and arrive at Aleppo, passing Korfu, Candia, Cyprus etc; since Venice, Korfu and Candia remain fixed and do not move with the ship. But in respect to the bales, chests, and other pieces of baggage which are on the ship as cargo or ballast, the movement of the ship itself from Venice to Syria is as good as non-existent, since their position in relation to one another does not change; and this is due to the fact that the movement is a common one in which they all take part. If of the wares on the ship one bale moves only an inch away from the chest, this is for it a

greater movement in relation to the chest, than the whole journey of 2,000 miles which they undergo in common.

Therefore, since plainly the motion which many movable bodies undergo in common is without effect and, with regard to their mutual position toward one another, it is as if it did not exist, for there is no change among them; and since it only affects the relative position of such bodies as do not share in the movement, for in this case the mutual relation is changed; since we have divided the universe into two parts, of which one must be movable and the other immovable; then for all purposes this movement will be of the same effect whether it is ascribed to the earth alone or to all the rest of the universe. For the working of such a motion is on nothing but the relative position in which the earth and the heavenly bodies stand to one another, and aside from this relative position nothing changes. If now it is indifferent for accomplishing this result whether the earth alone moves and the whole universe rests, or the earth rests and the whole universe is subject to one common movement, who can believe that Nature—who by common agreement does not employ great means when she can obtain the same result by smaller ones—would have undertaken to set in motion an immeasurable number of mighty bodies, and that with incredible velocity, to accomplish what could be obtained by the moderate motion of one single body around the center?

SIMPL.: I do not agree that that mighty movement would be as if it did not happen in regard to the sun, the moon, the innumerable host of fixed stars. Do you call it nothing that the sun goes from one meridian to another, rises from one horizon, sinks under another, brings now day, now night; that the moon goes through similar changes and likewise the other planets, as well as the fixed stars?

SALV.: All the changes mentioned by you are such only with respect to the earth. To demonstrate this, only imagine yourself away from the earth; there is then no rising or setting of the sun, no horizons, no meridians, no day, no night; in a word, by the movement mentioned no change in the relation of the moon to the sun or to any other star is evoked. All these changes have reference to the earth; they are supposed only because the sun is first visible in China, then Egypt, Greece, France, Spain, America, and so on, and so also for the moon and the other heavenly bodies. The same proc-

ess would occur in the same way, if, without disturbing so vast a part of the universe, the earth alone should be revolved.

The difficulty is however doubled since a second very important one is added. That is, if one attributes to the firmament this mighty motion, one must regard it as necessarily opposed to the particular movements of all the planets, all of which indisputably have their own movements from west to east, and in comparison very moderate movements at that. One is then forced to the conclusion that they depart from that rapid daily motion, namely from east to west, to go in the opposite direction. But, if we suppose that the earth moves, the opposition of motions disappears and the single movement from west to east fits in with all the facts and explains them most satisfactorily.

SIMPL.: As far as this opposition of motions is concerned that has little importance, since Aristotle proves that the circular motions are not opposed to one another and that the apparent opposition cannot actually be called so.

SALV.: Does Aristotle prove that or merely suppose it, because it aids him for a certain purpose? If, according to his own declaration, those things are opposed which mutually destroy one another I do not see how two moving bodies which meet one another in a circular motion should do one another less harm than if they meet on a straight line.

SAGR.: Wait a moment, I pray. Tell me, Signore Simplicio, if two knights run into one another with leveled lances on the open field, if two squadrons or two streams on their way to the sea break through and unite with one another, would you call such collisions opposed movements?

SIMPL.: Of course we would call them opposed.

SAGR.: How then is there no opposition in circular motions? For the movements mentioned take place upon the surface of the earth or water, both of which are recognized to be circular in form and so the motions must be circular. Do you understand, Signore Simplicio, what circular motions are not opposed to one another? Two circles which touch each other on the outside and of which the revolution of one is in a reverse direction from that of the other. If, however, one circle is within the other, then motions in different directions must be opposed to one another.

SALV.: Whether opposed or not opposed is merely a strife of

words. I know that in fact it is simpler and more natural to accomplish everything with one motion than to call in two. If you do not wish to call them opposite, then call them reverse. Moreover, I mention this introduction of a double movement not as something impossible, and in no way propose to deduce from it a strong proof for the motion of the earth, but merely a high degree of probability for it.

The improbability of the movement of the universe about the earth is tripled, however, by the complete upsetting of that arrangement which governs all the heavenly bodies whose circular motion is accepted not doubtfully but with full assurance. That is, that in such cases the larger the orbit the longer the time required for its completion, and the smaller, the shorter. Saturn, whose course surpasses all the planets in extent, completes it in thirty years. Jupiter revolves in a smaller circle in twelve years. Mars in two, the moon in a month. We see clearly in the case of the Medicean stars [the moons of Jupiter] that the one nearest Jupiter goes through its orbit in a very short time, namely, forty-two hours, the next nearest in three and a half days, the third in seven days, and the farthest removed in sixteen days. This thoroughly constant rule remains unchanged if we ascribe the twenty-four hour movement to the revolution of the earth, but if we suppose the earth to remain unmoved, we must proceed from the short period of the moon to increasingly greater periods, to the two year period of Mars, the twelve year period of Jupiter, the thirty year period of Saturn, and then abruptly to a disproportionately larger orbit, to which must also be ascribed the revolution in twenty-four hours. And these suppositions entail the smallest part of the disturbance of the otherwise constant law. For when one passes from the orbit of Saturn to those of the fixed stars and attributes to them even greater orbits, which correspond to the period of revolution of many thousands of years, one must pass from this by a much more disproportionate transition to that other movement and ascribe to them a period of revolution about the earth of twenty-four hours. But if the movement of the earth is supposed, the regularity of the period is accounted for in the best possible way; from the slow period of Saturn we arrive at the immovable fixed star.

A fourth difficulty also is encountered which must be added if we suppose the motion of the smaller sphere. I mean the great dissimilarity in movements of these stars, some of which must revolve

at a tremendous rate in immense circles, others slowly in smaller circles, according as they are placed at greater or smaller distances from the pole. And not only the size of the different circles and so the velocity of movement varies greatly in different fixed stars, but also the same stars change their courses and their velocity; herein is the fifth difficulty. That is, those stars which 2,000 years ago stood on the equator of the stellar sphere and thereafter moved in the greatest circles, must now, since to-day they have moved several degrees from it, move more slowly and in smaller circles. Within a conceivable time it will happen that one of those which have been continually moving will eventually reach the pole and cease to revolve, then later, after a period of rest, begin to move again. The other stars, however, which undoubtedly move, all have, as has been said, as orbit an immense circle and move in it without change.

The improbability is increased (and this may be called a sixth difficulty) for him who investigates basic principles, by the fact that one cannot imagine the firmness which that immense sphere must possess, in whose depths so many stars are so solidly fixed that in spite of such varieties of motions they are held together in the revolution without in any way changing their relative positions. But if according to the most probable view the heavens are fluid, so that each star may describe its own orbit, by what law and according to what principles are their orbits governed, so that seen from the earth they appear as if held in one sphere? To accomplish this it seems to me it would be easier and more convenient to make them stationary instead of movable, just as the paving stones in the market place are kept in order more easily than the troops of children who race over them.

Finally the seventh objection; if we ascribe the daily revolution to the highest heavens we must suppose this to be of such power and force that it bears along the innumerable crowd of fixed stars, every one a body of immense mass and much larger than the earth, further, all the planets, although these by their nature move in an opposite direction. Moreover, we must suppose that the element of fire and the greater portion of the air is also borne along; therefore, singly and alone the little earth ball withstands stubbornly and independently this mighty force: a supposition that seems to me to have much against it. I cannot explain how the earth, a body freely suspended and balanced

on its axis, inclined by nature as much toward motion as the rest, surrounded by a fluid medium, is not seized on by this general revolution. We do not encounter this difficulty, however, if we suppose the earth to move, a body so small, so inconsiderable in comparison with the whole universe that it could have no effect at all upon this.

V

WILLIAM HARVEY

1578-1657

In 1615 William Harvey stated his theory of the circulation of the blood, which he derived from patient observations, in his lectures on anatomy. The theory was epoch-making in the history of physiology because it initiated the study of the chemical constituency of the blood and of its function in nutrition.

Harvey, born April 1, 1578, in the south of England, attended the University of Cambridge, and took his degree in 1597. The following four years he studied at Padua under Fabricius. In 1602, when he returned to England, he began the practice of medicine, and in 1609 became connected with St. Bartholomew's Hospital. He published his "Excercitatio" in 1628, served for several years as physician to Charles I, and retired in 1646 to private life. He died June 3, 1657.

He described the process of his discovery as follows: "I frequently and seriously bethought me, and long revolved in my mind, what might be the quantity of blood which was transmitted, in how short a time its passage might be effected, and the like; and not finding it possible that this could be supplied by the juices of the ingested aliment without the veins on the one hand being drained, and the arteries on the other hand becoming ruptured through the excessive charge of blood, unless the blood should somehow find its way from the arteries into the veins, and so return to the right side of the heart; I began to think whether there might not be a motion, as it were, in a circle. Now this I afterwards found to be true; and I finally saw that the blood, forced by the action of the left ventricle into the arteries, was distributed to the body at large, and its several parts, in the same manner as it is sent through the lungs, impelled by the right ventricle into the pulmonary artery, and that it then passed through the veins and along the vena cava, and so round to the left

ventricle in the manner already indicated,—which motion we may be allowed to call circular.”

THE CIRCULATION OF BLOOD IN ANIMALS *

Thus far I have spoken of the passages of the blood from the veins into the arteries, and of the manner in which it is transmitted and distributed by the action of the heart; points to which some, moved either by the authority of Galen or Columbus, or the reasonings of others, will give in their adhesion. But what remains to be said upon the quantity and source of the blood which thus passes, is of so novel and unheard-of character, that I not only fear injury to myself from the envy of the few, but I tremble lest I have mankind at large for my enemies, so much doth wont and custom, that become as another nature, and doctrine once sown and that hath struck deep root, and respect for antiquity influence all men: Still the die is cast, and my trust is in my love of truth, and the candour that inheres in cultivated minds. And sooth to say, when I surveyed my mass of evidence, whether derived from vivisections, and my various reflections on them, or from the ventricles of the heart and the vessels that enter into and issue from them, the symmetry and size of these conduits,—for nature doing nothing in vain, would never have given them so large a relative size without a purpose,—or from the arrangement and intimate structure of the valves in particular, and of the other parts of the heart in general, with many other things besides, I frequently and seriously bethought me, and long revolved in my mind, what might be the quantity of blood that was transmitted, in how short a time its passage might be effected, and the like; and not finding it possible that this could be supplied by the juices of the ingested aliment without the veins on the one hand becoming drained, and the arteries on the other getting ruptured, through the excessive charge of blood, unless the blood should somehow find its way from the arteries into the veins, and so return to the right side of the heart; I began to think whether there might not be *A Motion, As It Were, In A Circle*. Now this I after-

*From *An Anatomical Disquisition on the Motion of the Heart-Blood in Animals*.

ward found to be true; and I finally saw that the blood, forced by the action of the left ventricle into the arteries, was distributed to the body at large, and its several parts, in the same manner as it is sent through the lungs, impelled by the right ventricle into the pulmonary artery, and that it then passes through the veins and along the vena cava, and so round to the left ventricle in the manner already indicated. Which motions we may be allowed to call circular, in the same way as Aristotle says that the air and rain emulate the circular motion of the superior bodies; for the moist earth, warmed by the sun, evaporates; the vapours drawn upwards are condensed, and descending in the form of rain, moisten the earth again; and by this arrangement are generations of living things produced; and in like manner too are tempests and meteors engendered by the circular motion, and by the approach and recession of the sun.

And so, in all likelihood, does it come to pass in the body, through the motion of the blood; the various parts are nourished, cherished, quickened by the warmer, more perfect, vaporous, spiritous, and, as I may say, alimentive blood; which, on the contrary, in contact with these parts becomes cooled, coagulated, and, so to speak, effete; whence it returns to its sovereign the heart, as if to its source, or to the inmost home of the body, there to recover its state of excellence, or perfection.

Here it resumes its due fluidity and receives an infusion of natural heat—powerful, fervid, a kind of treasury of life, and is impregnated with spirits, and it might be said with balsam; and thence it is again dispersed; and all this depends on the motion and action of the heart.

The heart, consequently, is the beginning of life; the sun of the microcosm, even as the sun in his turn might well be designated the heart of the world; for it is the heart by whose virtue and pulse the blood is moved, perfected, made apt to nourish, and is preserved from corruption and coagulation; it is the household divinity which, discharging its function, nourishes, cherishes, quickens the whole body, and is indeed the foundation of life, the source of all action.

VI

ROBERT BOYLE

1627-1691

Robert Boyle, fourteenth child of the Earl of Cork, was born January 25, 1627, in Munster, Ireland. He went to Eton, studied under the rector of Stalbridge, and later traveled on the Continent under private tutors. On the death of his father in 1644, he inherited the manor at Stalbridge. At the age of eighteen he became associated with the English scientific investigators at Oxford who later founded the Royal Society, and engaged actively in physical experiments and researches. The greatest of his achievements was his discovery of the law of the compressibility of gases. He died December 30, 1691.

THE DISCOVERY OF THE LAW OF THE COMPRESSIBILITY OF GASES *

We took a long glass tube, which, by a dexterous hand and the help of a lamp, was in such a manner crooked at the bottom, that the part turned up was almost parallel to the rest of the tube, and the orifice of this shorter leg of the syphon (if I may so call the whole instrument) being hermetically sealed, the length of it was divided into inches (each of which was subdivided into eight parts) by a straight list of paper, which, containing those divisions, was carefully pasted all along it. Then putting in as much quicksilver as served to fill the arch or bended part of the syphon, that the mercury standing in a level might reach in one leg to the bottom of the divided paper, and just to the same height or horizontal line in the other, we took care, by frequently inclining the tube, so that the air might freely pass

* From Thorpe, *Essays on Historical Chemistry*.

from one leg into the other by the sides of the mercury (we took, I say, care), that the air at last included in the shorter cylinder should be the same laxity with the rest of the air about it. This done, we began to pour quicksilver into the longer leg of the syphon, which, by its weight pressing up that in the shorter leg, did by degrees straighten the included air; and continuing this pouring in of quicksilver till the air in the shorter leg was by condensation reduced to take up but half the space it possessed (I say possessed, not filled) before, we cast our eyes upon the longer leg of the glass, upon which we likewise pasted a slip of paper carefully divided into inches and parts, and we observed, not without delight and satisfaction, that the quicksilver in that longer part of the tube was 29 inches higher than the other. Now that this observation does both very well agree with and confirm our hypothesis, will be easily discerned by him that takes notice what we teach: and Monsieur Pascal and our English friend's [Mr. Townley's] experiments prove, that the greater the weight is that leans upon the air, the more forcible is its endeavor of dilation, and consequently its power of resistance (as other springs are stronger when bent by greater weights). For this being considered, it will appear to agree rarely well with the hypothesis, that as according to it the air in that degree of density, and correspondent measure of resistance, to which the weight of the incumbent atmosphere had brought it, was unable to counterbalance and resist the pressure of a mercurial cylinder of about 29 inches, as we are taught by the Torricellian experiment; so here the same air being brought to a degree of density about twice as great as that it had before, obtains a spring twice as strong as formerly. As may appear by its being able to sustain or resist a cylinder of 29 inches in the longer tube, together with the weight of the atmospherical cylinder that leaned upon those 29 inches of mercury; and, as we just now inferred from the Torricellian experiment, was equivalent to them.

(The tube broke at this point and, unable to proceed after several similar efforts, Boyle tried the converse experiment—to determine the spring of rarefied air. A tube, about 6 feet in length, and sealed at one end, was nearly filled with mercury, and into it was placed)—

A slender glass pipe of about the bigness of a swan's quill, and open at both ends; all along of which was pasted a narrow list of paper, divided into inches and half-quarters. This slender pipe be-

ing thrust down into the greater tube almost filled with quicksilver, the glass helped to make it swell to the top of the tube; and the quicksilver getting in at the lower orifice of the pipe filled it up till the mercury included in that was near about a level with the surface of the surrounding mercury in the tube. There being, as near as we could guess, little more than an inch of the slender pipe left above the surface of the restagnant mercury, and consequently unfilled therewith, the prominent orifice was carefully closed with sealing-wax melted; after which the pipe was let alone for a while that the air, dilated a little by the heat of the wax, might, upon refrigeration, be reduced to its wonted density. And then we observed, by the help of the above-mentioned list of paper, whether we had not included somewhat more or somewhat less than an inch of air; and in either case we were fain to rectify the error by a small hole made (with a heated pin) in the wax, and afterward closed up again. Having thus included a just inch of air, we lifted up the slender pipe by degrees, till the air was dilated to an inch, an inch and a half, two inches, etc., and observed in inches and eighths the length of the mercurial cylinder, which, at each degree of the air's expansion, was impelled above the surface of the restagnant mercury in the tube. The observations being ended, we presently made the Torricellian experiment with the above mentiond great tube of 6 feet long, that we might know the height of the mercurial cylinder for that particular day and hour, which height we found to be $29\frac{3}{4}$ inches.

VII

CHRISTIAN HUYGHENS

1629-1695

Christian Huyghens was born at The Hague, April 14, 1629. He studied law in Breda, but becoming attracted to the study of mathematics he neglected his legal practice for it. In 1655 he improved the method of grinding telescopic lenses, and, assisted by his brother, discovered the sixth satellite of Saturn and the fact that it was belted with rings. In 1657 he presented to the States-General the first pendulum clock. In 1678 he evolved his wave theory of light, and published it at Leyden in 1690. He died at The Hague, June 8, 1695.

THE WAVE THEORY OF LIGHT *

Proofs in optics, as in every science in which mathematics is applied to matter, are founded upon facts from experience—as for example, that light moves in straight lines, that the angles of incidence and reflection are equal, and that light rays are refracted in accordance with the law of sines [i. e., that the ratio between the sines of the incident and refracted ray is constant for the same substance.] For this last law is now as generally and surely known as either of the others.

Most writers in optics have been content to assume these facts, but others more curious have attempted to discover the source and reason of these phenomena, looking upon them as being in themselves interesting data. Yet although they have propounded some ingenious theories, intelligent readers still require a fuller explanation before being entirely satisfied. Therefore I herein offer some considerations on the matter with the hope of making clearer this branch of physics which has not improperly gained the reputation of being very obscure.

I feel myself particularly indebted to those that first began to study

* Translated from *Traité de la Lumière*.

these profound subjects, and to lead us to hope them capable of orderly explanation. Yet I have been surprised to find these very investigators accepting arguments far from clear as if proof conclusive. No one has yet offered even a probable explanation of the first two remarkable phenomena of light,—why it moves in straight lines, and why rays from any and all directions can cross one another without interference.

I shall attempt in this treatise to submit clearer and more probable reasons, along the lines of modern philosophy, first for the transmission of light, and, second, for its reflection when it meets certain bodies.

Further, I shall explain the fact of rays said to undergo refraction in passing through various transparent bodies. Here I shall consider also, the refractions due to the differing densities of the atmosphere. Later I shall investigate the remarkable refraction occurring in Icelandic crystals. Finally, I shall study the different shapes necessary in transparent and reflecting bodies in order to bring together rays upon a single point or to deflect them in different ways. Here we shall see how easy it is by our new theory to determine not alone the ellipses, hyperbolas, and other curves which M. Descartes has so shrewdly constructed for this end, but as well the curve that one surface of a lens must have when the other surface is known, as spherical, plane, or any other figure.

We cannot but believe that light is the motion of a certain material. Thus when we reflect on its production, we discover that here on the earth it is usually emitted from fire and flame, and that these unquestionably contain bodies in rapid motion, since they can soften and melt many other more solid substances. If we note its effects, we see that when light is brought to a point, as, for example, by concave mirrors, it can cause combustion the same as fire: that is, it can force bodies apart, a power that certainly argues motion, at least in that true science where one believes all natural phenomena to result from mechanical causes. Moreover, in my mind we must either admit this or give up all hope of ever understanding anything in natural science.

Since, according to this philosophy, it is believed certain that the sensation of sight is produced only by the impulse of some form of matter against the nerves at the base of the eye, we have yet another reason for believing light to be a motion in the substance lying between us and the body producing the light.

As soon as we consider, moreover, the enormous speed with which light travels in every direction, and the fact that when rays come from different directions, even from those exactly opposite, they cross without interference, it must be plain that we do not see luminous objects by means of particles transmitted from the objects to us, as a shot or an arrow moves through the air. For surely this would not allow for the two qualities of light just mentioned, particularly the latter (that of speed). Light, then, is transmitted in some other way, a comprehension of which we may get from our knowledge of how sound moves through the air.

We know that sound is sent out in all directions through the medium of the air, a substance invisible and impalpable, by means of a motion that is communicated successively from one part of the air to the next; and as this movement has the same speed in all directions, it must form spherical surfaces that keep enlarging until at last they strike the ear. Now there can be no doubt that light likewise reaches us from a luminous substance through some motion caused in the matter lying in the intervening space,—for we have seen above that this cannot take place through transmission of matter from one place to another.

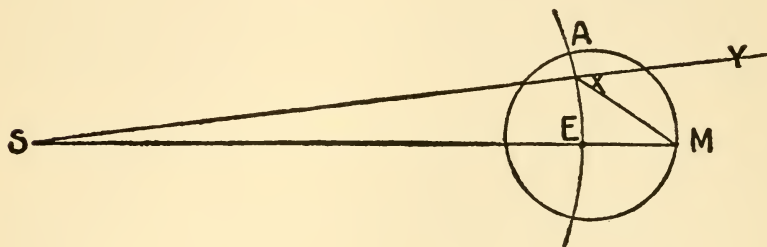
If, moreover, light requires time for its passage—a matter we shall discuss in a moment—it will then follow that this movement is caused in the substance gradually, and therefore is transmitted, like sound, by surfaces and spherical waves. I call these *waves* because of their likeness to those formed when one throws a pebble into water, which are examples of gradual propagation in circles, although from a different cause and on a plane surface.

In regard to the question of light requiring time for its transmission, let us consider whether there is any experimental evidence against it.

What experiments we can make here on the earth with sources of light placed at great distances (although indicating that it does not take a sensible time for light to pass over these distances) are subject to the objection that these distances are yet too small, and that we can only argue that the movement of light is enormously fast. M. Descartes thought it to be instantaneous and based his opinion upon much better reasons taken from the eclipse of the moon. Yet as I shall make clear, even this evidence is not decisive. I shall state the matter

in a somewhat different way from his in order more easily to exhibit all the consequences.

Suppose S to be the position of the sun, E A part of the orbit of the earth, $S E M$ a straight line intersecting in M , the orbit of the moon, represented by the circle $A M$.



Now if light requires time—say an hour—to move the distance between the earth and the moon, then [at the time of an eclipse] it follows that when the earth has come to E its shadow, or the stoppage of the light of the sun, will not yet have reached M [the moon], and will not for an hour. Counting from the instant the earth reaches E , it will be an hour before it will reach M if it is to be obscured there. This eclipse will not be seen from the earth for yet another hour. Suppose that during these two hours the earth has moved to X , the moon appearing eclipsed at M , the sun still being seen at S . For I assume as does Copernicus that the sun is fixed and since light moves in straight lines, is always seen in its true position.

But as a matter of fact, we are assured that the eclipsed moon always appears directly opposite the sun; while on the above supposition [that light takes an hour in passing between the moon and the earth], its position ought to be back of the straight line by the angle $Y X M$, the supplement of the angle $S X M$. But this is not the case, for this angle $Y X M$ would be very easily noticed, it being about 33 degrees. For by our analysis (found in the essay on the causes of the phenomena of Saturn), the distance from the sun to the earth, $S E$, is about 12,000 times the diameter of the earth, and hence 400 times the distance of the moon, which is 30 diameters.

The angle XME then will be nearly 400 times as great as ESX , which is 5 minutes, i. e., the angular distance travelled by the earth in two hours [the earth traversing almost a degree in a day]. Thus the angle EMX is almost 33 degrees, and likewise the angle $MX Y$, being 5 minutes greater [than EMX].

Now it must be remembered that in this computation it is assumed that the speed of light is such as to consume an hour in passing from here to the moon. But if we assume it to take only a minute of time, then the angle YXM would amount to only 33 minutes, and if it only takes ten seconds, this angle will be less than six minutes. Now so small an angle is not observable in a lunar eclipse and hence it is not permissible to argue that light is absolutely instantaneous.

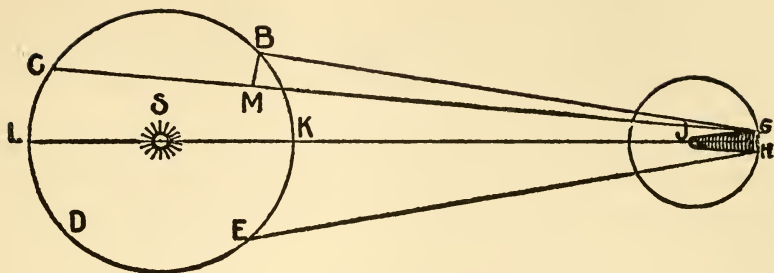
It is rather unusual, we admit, to take for granted a speed 100,000 times as great as that of sound, which (following my experiments) travels about 180 toises [about 1150 feet] in a second, or during a pulse-beat. Yet this supposition is not at all impossible, for it is not necessary to carry a body at such speed but only for motion to traverse successively from one point to another.

Hence I do not hesitate in this matter to assume that the passage of light takes time, for on this assumption all phenomena can be explained, while on the contrary supposition none of them can be explained. In fact, it seems to me and to many others as well, that M. Descartes, whose purpose has been to discuss all physical matters clearly, and who has certainly succeeded in this better than any one before him, has written nothing on light and its qualities that is not either hard to understand or even incomprehensible.

Moreover, this idea that I have propounded as an hypothesis has lately been made a well nigh established fact by that keen calculation of Roemer, whose method I will here take occasion to describe, on the expectation that he will himself in the future fully confirm this theory.

His method, the same as the one we have just discussed, is astronomical. He shows not only that light takes time for its passage, but calculates also its speed and that this must be at least six times as much as the rate I have just given as an estimate.

In his demonstration he uses the eclipses of the small satellites that revolve around Jupiter, and very frequently pass into his shadow. Roemer's reasoning is this:



Let S be the sun, B C D E the yearly orbit of the earth, J Jupiter and G H the orbit of his nearest satellite, for this one because of its short period is better suited to this investigation than any one of the other three. Suppose G to be the point where the satellite enters, and H where it leaves, Jupiter's shadow.

Suppose that when the earth is at B, the satellite is seen to emerge [at G], at some time before the last quarter. Were the earth to remain stationary there, $42\frac{1}{2}$ hours would elapse before the next emergence would take place, for this much time is taken by the satellite in making one revolution in its orbit and returning to opposition to the sun. For example, if the earth remained at B during 30 revolutions, then after 30 times $42\frac{1}{2}$ hours, the satellite would again be seen to emerge. If in the meantime the earth has moved to C, farther from Jupiter, it is clear that if light requires time for its passage, the emergence of the satellite will be seen later when the earth is at C than when at B. For we must add to the 30 times $42\frac{1}{2}$ hours, the time occupied by light in passing over the difference between the distances [of the earth from Jupiter] G B and G C, i. e., M C. So in the other quarter, when the earth travels from D to E, approaching Jupiter, the eclipses will occur earlier when the earth is at E than when at D.

Now by many observations of these eclipses throughout ten years, it is shown that these inequalities are actually of some moment, amounting to as much as ten minutes or more: whence it is argued that in traversing the whole diameter of the earth's orbit, K L, double the distance from the earth to the sun, light takes about 22 minutes.



The motion of Jupiter in its orbit while the earth passes from B to C or from D to E has been taken into consideration in Roemer's calculation, where it is also proved that these inequalities cannot be caused by any irregularity or eccentricity in the movement of the satellite.

Now if we consider the enormous size of this diameter K L [the earth's orbit] which I have estimated to be about 24,000 times that of the earth, we get some comprehension of the extraordinary speed of light.

Even if K L were only 22,000 diameters of the earth, a speed traversing this distance in 22 minutes would be equal to the rate of a thousand diameters a minute, i. e., 16 2-3 diameters a second (or a pulse-beat) which makes more than 1,100 times 100,000 toises, since one diameter of the earth equals 2,865 leagues, of which there are 25 to the degree, and since in accordance with the very precise calculation made by M. Picard in 1609 under orders from the king, each league contains 2,282 toises.

As I stated before sound moves only 180 toises per second. Hence the speed of light is over 600,000 times as great as that of sound, which, however, is very different from being instantaneous,—it is the difference between any finite number and infinity. The theory that light movements are propagated from point to point in time being thus demonstrated, it follows that light moves in spherical waves, as does sound.

But if they are alike in this regard, they are unlike in others, as in the original cause of the motion that transmits them, the medium through which they move, and the manner in which they are transmitted in it.

We know that sound is caused by the rapid vibration of some body (either as a whole or in part), this vibration setting in motion the adjoining air. But light movements must arise at every point of the luminous body, otherwise all the various parts of the body would not be visible. This fact will be clearer from what follows.

In my judgment, this movement of light-giving bodies cannot be more satisfactorily explained than by supposing that those that are fluid, e. g., a flame, and probably the sun and stars, consist of particles that float about in a much rarer medium, that sets them in violent motion, causing them to strike against the still more minute particles

of the surrounding ether. In the case of light-giving solids such as red-hot metal or carbon we may suppose this movement to be caused by the rapid motions of the metal or wood, the particles on the surface exciting the ether. Hence the vibration producing light must be much shorter and faster than that causing sound, since we do not find that sound disturbances give rise to light any more than the wave of the hand through the air causes sound.

The next question is in regard to the nature of the medium through which the vibration produced by light-giving bodies moves. I have named it *ether*, but it plainly differs from the medium through which sound moves. The latter is simply the air we feel and breathe, and when it is removed from any space, the medium which carries light still remains. This is shown by surrounding the sounding body in a glass vessel, and exhausting the air by means of the air-pump that Mr. Boyle has devised, and with which he has performed so many striking experiments. In trying this experiment, however, it is best to set the sounder on cotton or feathers so that it cannot communicate vibrations to the glass receiver or the air-pump, a point hitherto neglected. Then, when all the air has been exhausted, one catches no sound from the metal when it is struck.

Hence we conclude not only that our atmosphere which cannot penetrate glass is the medium through which sound acts, but that the medium carrying light-vibrations is something different: for after the vessel is exhausted of air, light passes through it as easily as before.

The last point is proven even more conclusively by the famous experiment of Torricelli. [Fill a long closed glass tube with mercury, then invert it.] The top of the glass tube not filled by the mercury contains a high vacuum, but transmits light as well as when filled with air. This demonstrates that there is within the tube some form of matter different from air, and which penetrates either glass or mercury, or both, though both are impenetrable to air. And if a like experiment is tried with a little water on top of the mercury, it becomes equally clear that the substance in question traverses either glass or water or both.

In regard to the different methods of transmission of sound and light, in the case of sound it is easy to see what happens when one remembers that air can be compressed and reduced to a much smaller volume than usual, and that it tends with the same force to expand to

its original volume. This quality, considered along with its penetrability retained in spite of such condensation seems to show that it consists of small particles that float about in rapid vibration in an ether consisting of still more minute particles. Sound, then, is caused by the struggle of these particles to escape when at any point in the course of a wave they are more crowded together than at some other point.

Now the wonderful speed of light considered with its other qualities, does not permit us to believe it to be transmitted in the same manner. Therefore I shall try to explain the way in which I think it must take place. I must first, however, describe that quality of hard substances through which they transmit motion one to another. If one take a number of balls of the same size of any hard substance, and place them touching one another in one line, he will find that on letting a ball of the same size strike against one end of the line, the motion is transmitted in an instant to the other end of the line. The last ball is driven from the line while the others are apparently undisturbed, the ball that struck the line coming to a dead stop. This is an illustration of a transmission of motion at great speed, varying directly as the hardness of the balls. Yet it is certain that this transmission is not instantaneous, but requires time. For if the movement, or if you wish, the tendency to move, did not pass from one ball to another in succession, they would all be set in motion at the same instant and would all move forward at the same time. Now this is so far from the case that only the last one leaves the row, and it has the speed of the ball that first struck the line.

There are other experiments, also demonstrating that all bodies, even those thought hardest, such as steel, glass and agate, are really elastic, and bend a little, no matter whether they are in rods, balls, or bodies of any other shape,—that is, they give slightly at the point where struck, and at once regain their former shape. Thus I have discovered that in letting a glass or agate ball strike on a large, thick, flat piece of the same substance the surface of which has been roughened by the breath, the place where it strikes is shown by a circular indentation that varies in size directly as the force of the blow. This indicates that the materials give when struck and then fly back,—an event that necessarily takes time.

Now to apply such a motion to the explanation of light, there is

nothing in the way of our imagining the particles of ether to have an almost complete hardness, and an elasticity as perfect as we need wish. We need not here discuss the cause of either this hardness or elasticity, as this would lead us too far from the question at issue. I will remark, however, by the way, that these particles of ether, in spite of their minuteness, are also composed of parts and that their elasticity depends on a very rapid motion of a subtle substance traversing them in all directions and making them take a structure that offers a ready passage to this fluid. This agrees with the idea of M. Descartes, except that I would not, like him, give the pores the shape of round, hollow canals. This is so far from being at all absurd or incomprehensible that it is easily credible that nature uses an infinite series of different-sized molecules in order to produce her marvelous effects.

Moreover, although we do not know the cause of elasticity, we cannot have failed to notice that most bodies possess this characteristic; hence it is not unreasonable to suppose that it is a quality of the minute, invisible particles of the ether. And it is a fact that if one looks for some other method of accounting for the gradual transmission of light, he will have a hard time finding any supposition better suited than elasticity to explain the fact of uniform speed. This [uniform speed] seems to be a necessary assumption, for if the motion slowed down when distributed over a great mass of matter at a far distance from its source, then this great speed would at last be lost. On the other hand, we suppose ether to have the property of elasticity so that its particles regain their shape with equal activity whether struck a hard or gentle blow. Thus the rate at which light would move would remain constant.

VIII

ANTHONY VAN LEEUWENHOECK

1632-1723

Born in Delft, Holland, October 24, 1632, Anthony Van Leeuwenhoeck, a lens-maker for microscopes, made several important biological discoveries. In 1673 he noticed the red globules in the blood; in 1675 he discovered animalculæ in water; in 1677 he described the spermatozoa; in 1690 he traced the passage of blood from the arteries into the veins. Among his other achievements were his investigations of the tubules of teeth, the solidity of hair, the structure of the epidermis, and his descriptions of insect anatomies. He announced most of his findings to the Royal Society of London. Against the generally accepted idea of spontaneous generation, he held that all things generated their kind. He died at Delft, August 26, 1723.

OBSERVATIONS ON ANIMALCULÆ *

In the year 1675, I discovered very small living creatures in rain water, which had stood but few days in a new earthen pot glazed blue within. This invited me to view this water with great attention, especially those little animals appearing to me ten thousand times less than those represented by M. Swammerdam, and by him called water-fleas, or water-lice, which may be perceived in the water with the naked eye.

The first sort I several times observed to consist of 5, 6, 7, or 8 clear globules without being able to discern any film that held them together, or contained them. When these animalcula or living atoms moved, they put forth two little horns, continually moving. The

* From the *Transactions of the Royal Society of London.*

space between these two horns was flat, though the rest of the body was roundish, sharpening a little towards the end, where they had a tail, near four times the length of the whole body, of the thickness, by my microscope, of a spider's web; at the end of which appeared a globule of the size of one of those which made up the body. These little creatures, if they chanced to light on the least filament or string, or other particle, were entangled therein, extending their body in a long round, and endeavoring to disentangle their tail. Their motion of extension and contraction continued a while; and I have seen several thousands of these poor little creatures, within the space of a grain of gross sand, lie fast clustered together in a few filaments.

I also discovered a second sort, of an oval figure; and I imagined their head to stand on a sharp end. These were a little longer than the former. The inferior part of their body is flat, furnished with several extremely thin feet, which moved very nimbly. The upper part of the body was round, and had within 8, 10, or 12 globules, where they were very clear. These little animals sometimes changed their figure into a perfect round, especially when they came to lie on a dry place. Their body was also very flexible; for as soon as they struck against the smallest fibre or string, their body was bent in, which bending presently jerked out again. When I put any of them on a dry place, I observed that, changing themselves into a round, their body was raised pyramidal-wise, with an extant point in the middle; and having laid thus a little while, with a motion of their feet, they burst asunder, and the globules were presently diffused and dissipated, so that I could not discern the least thing of any film, in which the globules had doubtless been enclosed; and at this time of their bursting asunder, I was able to discover more globules than when they were alive.

I observed a third sort of little animals, that were twice as long as broad, and to my eye eight times smaller than the first. Yet I thought I discerned little feet, whereby they moved very briskly, both in round and straight line.

There was a fourth sort, which were so small that I was not able to give them any figure at all. These were a thousand times smaller than the eye of a large louse. These exceeded all the former in celerity. I have often observed them to stand still as it were on a point, and then turn themselves about with that swiftness, as we see a

top turn round, the circumference they made being no larger than that of a grain of small sand, and then extending themselves straight forward, and by and by lying in a bending posture. I discovered also several other sorts of animals; these were generally made up of such soft parts, as the former, that they burst asunder as soon as they came to want water.

May 26, it rained hard; the rain growing less, I caused some of that rain-water running down from the house top, to be gathered in a clean glass, after it had been washed two or three times with water. And in this I observed some few very small living creatures, and seeing them, I thought they might have been produced in the leaded gutters in some water that had remained there before.

I perceived in pure water, after some days, more of those animals, as also some that were somewhat larger. And I imagine, that many thousands of these little creatures do not equal an ordinary grain of sand in bulk; and comparing them with a cheese-mite, which may be seen to move with the naked eye, I make the proportion of one of these small water-creatures to a cheese-mite to be like that of a bee to a horse; for, the circumference of one of these little animals in water is not so large as the thickness of a hair in a cheese-mite.

In another quantity of rain-water, exposed for some days to the air, I observed some thousands of them in a drop of water, which were of the smallest sort that I had seen hitherto. And in some time after I observed, besides the animals already noted, a sort of creatures that were eight times as large, of almost a round figure; and as those very small animalcula swam gently among each other, moving as gnats do in the air, so did these larger ones move far more swiftly, tumbling round as it were, and then making a sudden downfall.

In the waters of the river Maese I saw very small creatures of different kinds and colours, and so small, that I could very hardly discern their figures; but the number of them was far less than those found in rain-water. In the water of a very cold well in the autumn, I discovered a very great number of living animals very small, that were exceedingly clear, and a little larger than the smallest I ever saw. In sea-water I observed at first, a little blackish animal, looking as if it had been made up of two globules. This creature had a peculiar motion, resembling the skipping of a flea on white paper;

so that it might very well be called a water-flea; but it was far less than the eye of that little animal, which Dr. Swammerdam calls the water-flea. I also discovered little creatures therein that were clear, of the same size with the former animal, but of an oval figure, having a serpentine motion. I further noticed a third sort, which were very slow in their motion; their body was of a mouse colour, clear toward the oval point; and before the head and behind the body there stood out a sharp little point angle-wise. This sort was a little larger. But there was yet a fourth somewhat longer than oval. Yet of all these sorts there were but a few of each. Some days after viewing this water, I saw 100 where before I had seen but one; but these were of another figure, and not only less, but they were also very clear, and of an oblong oval figure, only with this difference, that their heads ended sharper; and although they were a thousand times smaller than a small grain of sand, yet when they lay out of the water in a dry place, they burst in pieces and spread into three or four very little globules, and into some aqueous matter, without any other parts appearing in them.

Having put about one-third of an ounce of whole pepper in water, and it having lain about three weeks in the water, to which I had twice added some snow-water, the other water being in great part exhaled; I discerned in it with great surprise an incredible number of little animals, of divers kinds, and among the rest, some that were three or four times as long as broad; but their whole thickness did not much exceed the hair of a louse. They had a very pretty motion, often tumbling about and sideways; and when the water was let to run off from them, they turned round like a top; at first their body changed into an oval, and afterwards, when the circular motion ceased, they returned to their former length. The second sort of creatures discovered in this water, were of a perfect oval figure, and they had no less pleasing or nimble a motion than the former; and these were in far greater numbers. There was a third sort, which exceeded the two former in number, and these had tails like those I had formerly observed in rain-water. The fourth sort, which moved through the three former sorts, were incredibly small, so that I judged, that if 100 of them lay one by another, they would not equal the length of a grain of coarse sand; and according to this estimate,

1,000,000 of them could not equal the dimensions of a grain of such coarse sand. There was discovered a fifth sort, which had near the thickness of the former, but almost twice the length.

In snow-water, which had been about three years in a glass bottle well stopped, I could discover no living creatures; and having poured some of it into a porcelain tea-cup, and put therein half an ounce of whole pepper, after some days I observed some animalcula, and those exceedingly small ones, whose body seemed to me twice as long as broad, but they moved very slowly, and often circularly. I observed also a vast multitude of oval-figured animalcula, to the number of 8,000 in a single drop.

IX

SIR ISAAC NEWTON

1642-1727

Sir Isaac Newton, whose researches in light, gravitation, and mathematics are outstanding in the history of modern science, was born in Woolsthorpe, Lincolnshire, December 25, 1642. He was the son of an English farmer who died before Newton was born. His early education was interrupted by his mother's poverty, but his ingenuity in making mechanical toys soon provided a means whereby he was enabled to return to school. He entered Cambridge University in 1661 and took his degree in 1665; two years later he was made a fellow of the university, and in 1669 became professor of mathematics.

In 1665 he discovered his method of fluxions, not greatly unlike Leibnitz's Differential Calculus. In 1672 he was elected a fellow of the Royal Society and shortly afterwards sent them a paper describing how he had broken up light by means of a prism, demonstrating by that means the compound nature of the sun's rays.

In 1687 he elaborated his theory of gravitation in "Philosophiæ Naturalis Principia Mathematica." This was the result of his reflections and researches dating from 1666, when he attempted to explain the moon's motion by the hypothesis of the assumed influence of gravitation. In the meantime, through the use of telescopic instruments, French geographers had tested the spherical shape of the earth and had made a new and more accurate triangulation. Using the data which they supplied, Newton perceived that these data agreed with his theory that the force varied inversely as the square of the distance. Overcome with the emotion incident to the solution of a great problem, he begged a friend to complete his calculations, with the result that the new astronomy begun by Copernicus, and continued by Brahe, Kepler, and Galileo, was formulated and mathematically interpreted by a single mechanical principle.

Although he later made some chemical investigations, his papers were accidentally destroyed, and it is said that he never recovered from the shock of losing them. In 1695 he was made warden, and in 1699 promoted to the mastership of the mint, which office he retained at a munificent salary until his death on March 20, 1727.

THE THEORY OF GRAVITATION *

BOOK III. PROPOSITION V. THEOREM V. SCHOLIUM

The force which retains the celestial bodies in their orbits has been hitherto called centripetal force; but it being now made plain that it can be no other than a gravitating force, we shall hereafter call it gravity. For the cause of that centripetal force which retains the moon in its orbit will extend itself to all the planets.

BOOK III. PROPOSITION VI. THEOREM VI.

That all bodies gravitate towards every planet; and that the weights of bodies towards any the same planet, at equal distances from the centre of the planet, are proportional to the quantities of matter which they severally contain.

It has been, now of a long time, observed by others, that all sorts of heavy bodies (allowance being made for the inequality of retardation which they suffer from a small power of resistance in the air) descend to the earth *from equal heights* in equal times; and that equality of times we may distinguish to a great accuracy, by the help of pendulums. I tried the things in gold, silver, lead, glass, sand, common salt, wood, water, and wheat. I provided two wooden boxes, round and equal; I filled the one with wood, and suspended an equal weight of gold (as exactly as I could) in the centre of oscillation of the other. The boxes hanging by equal threads of 11 feet made a couple of pendulums perfectly equal in weight and figure, and equally receiving the resistance of the air. And, placing the one by the other, I observed them to play together forwards and backwards, for a long time, with equal vibrations. . . . and the like happened in the other bodies. By these experiments, in bodies of the same weight, I could manifestly have discovered a difference of

* Translated from the *Philosophiæ Naturalis Principia Mathematica*.

matter less than the thousandth part of the whole, had any such been. But, without all doubt, the nature of gravity towards the planets is the same as towards the earth. . . . Moreover, since the satellites of Jupiter perform their revolutions in times which observe the sesquiplicate proportion of their distances from Jupiter's centre—that is, equal at equal distances. And, therefore, these satellites, if supposed to fall *towards Jupiter* from equal heights, would describe equal spaces in equal times, in like manner as heavy bodies do on our earth. . . . If, at equal distances from the sun, any satellite, in proportion to the quantity of its matter, did gravitate towards the sun with a force greater than Jupiter in proportion to his, according to any given proportion, suppose of d to e ; then the distance between the centres of the sun and of the satellite's orbit would be always greater than the distance between the centres of the sun and of Jupiter nearly in the sub-duplicate of that proportion; as by some computations I have found. And if the satellite did gravitate towards the sun with a force, lesser in the proportion of e to d , the distance of the centre of the satellite's orbit from the sun would be less than the distance of the centre of Jupiter from the sun in the sub-duplicate of the same proportion. Therefore if, at equal distances from the sun, the accelerative gravity of any satellite towards the sun were greater or less than the accelerative gravity of Jupiter towards the sun but one 1–1000 part of the whole gravity, the distance of the centre of the satellite's orbit from the sun would be greater or less than the distance of Jupiter from the sun by one 1–2000 part of the whole distance—that is, by a fifth part of the distance of the utmost satellite from the centre of Jupiter; an eccentricity of the orbit which would be be very sensible. But the orbits of the satellite are concentric to Jupiter, and therefore the accelerative gravities of Jupiter, and of all its satellites towards the sun, are equal among themselves. . . .

But further; the weights of all the parts of every planet towards any other planet are one to another as the matter in the several parts; for if some parts did gravitate more, others less, than for the quantity of their matter, then the whole planet, according to the sort of parts with which it most abounds, would gravitate more or less than in proportion to the quantity of matter in the whole. Nor is it of any moment whether these parts are external or internal; for if, for example, we should imagine the terrestrial bodies with us to be

raised up to the orb of the moon, to be there compared with its body; if the weights of such bodies were to the weights of the external parts of the moon as the quantities of matter in the one and in the other respectively; but to the weights of the internal parts in a greater or less proportion, then likewise the weights of those bodies would be to the weight of the whole moon in a greater or less proportion; against what we have showed above.

Cor. 1. Hence the weights of bodies do not depend upon their forms and textures; for if the weights could be altered with the forms, they would be greater or less, according to the variety of forms in equal matter; altogether against experience.

Cor. 2. Universally, all bodies about the earth gravitate towards the earth; and the weights of all, at equal distances from the earth's centre, are as the quantities of matter which they severally contain. This is the quality of all bodies within the reach of our experiments; and therefore (by rule 3) to be affirmed of all bodies whatsoever. . . .

Cor. 5. The power of gravity is of a different nature from the power of magnetism; for the magnetic attraction is not as the matter attracted. Some bodies are attracted more by the magnet; others less; most bodies not at all. The power of magnetism in one and the same body may be increased and diminished; and is sometimes far stronger, for the quantity of matter, than the power of gravity; and in receding from the magnet decreases not in the duplicate but almost in the triplicate proportion of the distance, as nearly as I could judge from some rude observations.

BOOK III. PROPOSITION VII. THEOREM VII.

That there is a power of gravity tending to all bodies, proportional to the several quantities of matter which they contain.

That all the planets mutually gravitate one towards another, we have proved before; as well as that the force of gravity towards every one of them, considered apart, is reciprocally as the square of the distance of places from the centre of the planet. And thence (by prop. 69, book I, and its corollaries) it follows, that the gravity tending towards all the planets is proportional to the matter which they contain.

Moreover, since all the parts of any planet A gravitate towards any other planet B; and the gravity of every part is to the gravity of the whole as the matter of the part to the matter of the whole; and (by law 3) to every action corresponds an equal reaction; therefore the planet B will, on the other hand, gravitate towards all the parts of the planet A; and its gravity towards any one part will be to the gravity towards the whole as the matter of the part to the matter of the whole. Q. E. D.

Cor. 1. Therefore the force of gravity towards any whole planet arises from, and is compounded of, the forces of gravity towards all its parts. Magnetic and electric attractions afford us examples of this; for all attraction towards the whole arises from the attractions towards the several parts. The thing may be easily understood in gravity, if we consider a greater planet as formed of a number of lesser planets meeting together in one globe, for *hence it would appear* that the force of the whole must arise from the forces of the component parts. If it is objected that, according to this law, all bodies with us must mutually gravitate one towards another, I answer, that since the gravitation towards these bodies is to the gravitation towards the whole earth as these bodies are to the whole earth, the gravitation towards them must be far less than to fall under the observation of our senses.

Cor. 2. The force of gravity towards the several particles of any body is reciprocally as the square of the distance from the particles; as appears from cor. 3, prop. 74, book I.

X

BENJAMIN FRANKLIN

1706-1790

Benjamin Franklin, representative of the rationalist tendencies of the eighteenth century, was born in Boston, January 17, 1706. His early life and political missions are intimately related in his "Autobiography," a classic in American literature. Apart from his political services to the cause of American independence, he attained distinction in the field of scientific researches and experiments. In 1746 he began the experiments in electricity which resulted in his identification of electricity with lightning. He died in Philadelphia, April 17, 1790.

THE IDENTITY OF LIGHTNING AND ELECTRICITY *

But points have a property, by which they draw on as well as throw off the electrical fluid, at greater distances than blunt bodies can. That is, as the pointed part of an electrified body will discharge the atmosphere of that body, or communicate it farthest to another body, so the point of an unelectrified body will draw off the electrical atmosphere from an electrified body, farther than a blunter part of the same unelectrified body will do. Thus, a pin held by the head, and the point presented to an electrified body, will draw off its atmosphere at a foot distance; where, if the head were presented instead of the point, no such effect would follow. To understand this, we may consider, that, if a person standing on the floor would draw off the electrical atmosphere from an electrified body, an iron crow and a blunt knitting-needle, held alternately in his hand, and presented for that purpose, do not draw with different forces in proportion to their different masses. For the man, and what he holds in his hand, be it large or small, are connected with the common mass of unelectrified matter; and the force with which he draws

* From Franklin's correspondence with Peter Collinson, July 29, 1750. *Works of Benjamin Franklin*, Philadelphia, 1809, Vol. III, pp. 45-49.

is the same in both cases, it consisting in the different proportion of electricity in the electrified body, and that common mass. But the force, with which the electrified body retains its atmosphere by attracting it, is proportioned to the surface over which the particles are placed; that is, four square inches of that surface retain their atmosphere with four times the force that one square inch retains its atmosphere. And, as in plucking the hairs from the horse's tail, a degree of strength not sufficient to pull away a handful at once, could yet easily strip it hair by hair, so a blunt body presented cannot draw off a number of particles at once, but a pointed one, with no greater force, takes them away easily, particle by particle.

These explanations of the power and operation of points, when they first occurred to me, and while they first floated in my mind, appeared perfectly satisfactory; but now I have written them, and considered them more closely, I must own I have some doubts about them; yet, as I have at present nothing better to offer in their stead, I do not cross them out; for, even a bad solution read, and its faults discovered, has often given rise to a good one, in the mind of an ingenious reader.

Nor is it of much importance to us to know the manner in which nature executes her laws; it is enough if we know the laws themselves. It is of real use to know that China left in the air unsupported, will fall and break; but how it comes to fall, and why it breaks, are matters of speculation. It is a pleasure indeed to know them, but we can preserve our China without it.

Thus, in the present case, to know this power of points may possibly be of some use to mankind, though we should never be able to explain it. The following experiments, as well as those in my first paper, show this power. I have a large prime conductor, made of several thin sheets of clothier's pasteboard, formed into a tube, near ten feet long and a foot diameter. It is covered with Dutch embossed paper, almost totally gilt. This large metallic surface supports a much greater electrical atmosphere than a rod of iron of fifty times the weight would do. It is suspended by silk lines, and when charged will strike, at near two inches distance, a pretty hard stroke, so as to make one's knuckles ache. Let a person standing on the floor present the point of a needle, at twelve or more inches distance from it, and while the needle is so presented, the conductor cannot be charged, the point drawing off the fire as fast as it is thrown on by the

electrical globe. Let it be charged, and then present the point at the same distance, and it will suddenly be discharged. In the dark you may see the light on the point, when the experiment is made. And if the person holding the point stands upon wax, he will be electrified by receiving the fire at that distance. Attempt to draw off the electricity with a blunt body, as a bolt of iron round at the end, and smooth, (a silversmith's iron punch, inch thick, is what I use,) and you must bring it within the distance of three inches before you can do it, and then it is done with a stroke and crack. As the pasteboard tube hangs loose on silk lines, when you approach it with the punch-iron, it likewise will move towards the punch, being attracted while it is charged, but if, at the same instant, a point be presented as before, it retires again, for the point discharges it. Take a pair of large brass scales, of two or more feet beam, the cords of the scales being silk. Suspend the beam by a pack-thread from the ceiling, so that the bottom of the scales may be about a foot from the floor; the scales will move round in a circle by the untwisting of the pack-thread. Set the iron punch on the end upon the floor, in such a place as that the scales may pass over it in making their circle; then electrify one scale by applying the wire of a charged phial to it. As they move round, you see that scale draw nigher to the floor, and dip more when it comes over the punch; and, if that be placed at a proper distance, the scale will snap and discharge its fire into it. But, if a needle be stuck on the end of the punch, its point upward, the scale, instead of drawing nigh to the punch, and snapping, discharges its fire silently through the point, and rises higher from the punch. Nay, even if the needle be placed upon the floor near the punch, its point upward, the end of the punch, though so much higher than the needle, will not attract the scale and receive its fire, for the needle will get it and convey it away, before it comes nigh enough for the punch to act. And this is constantly observable in these experiments, that the greater quantity of electricity on the pasteboard tube, the farther it strikes or discharges its fire, and the point likewise will draw it off at a still greater distance.

Now if the fire of electricity and that of lightning be the same, as I have endeavoured to show at large in a former paper, this pasteboard tube and these scales may represent electrified clouds. If a tube of only ten feet long will strike and discharge its fire on the punch at

two or three inches distance, an electrified cloud of perhaps ten thousand acres may strike and discharge on the earth at a proportionately greater distance. The horizontal motion of the scales over the floor, may represent the motion of the clouds over the earth; and the erect iron punch, a hill or high building; and then we see how electrified clouds, passing over hills or high buildings at too great a height to strike, may be attracted lower till within their striking distance, And, lastly, if a needle fixed on the punch with its point upright, or even on the floor below the punch, will draw the fire from the scale silently at a much greater than the striking distance, and so prevent its descending towards the punch; or if in its course it would have come nigh enough to strike, yet being first deprived of its fire it cannot, and the punch is thereby secured from the stroke; I say, if these things are so, may not the knowledge of this power of points be of use to mankind, in preserving houses, churches, ships, &c., from the stroke of lightning, by directing us to fix, on the highest parts of those edifices, upright rods of iron made sharp as a needle, and gilt to prevent rusting, and from the foot of those rods a wire down the outside of the building into the ground, or down round one of the shrouds of a ship, and down her side till it reaches the water? Would not these pointed rods probably draw the electrical fire silently out of a cloud before it came nigh enough to strike, and thereby secure us from that most sudden and terrible mischief?

To determine the question, whether the clouds that contain lightning are electrified or not, I would propose an experiment to be tried where it may be done conveniently. On the top of some high tower or steeple, place a kind of sentry-box, . . . big enough to contain a man and an electrical stand. From the middle of the stand let an iron rod rise and pass bending out of the door, and then upright twenty or thirty feet, pointed very sharp at the end. If the electrical stand be kept clean and dry, a man standing on it, when such clouds are passing low, might be electrified and afford sparks, the rod drawing fire to him from a cloud. If any danger to the man should be apprehended (though I think there would be none), let him stand on the floor of his box, and now and then bring near to the rod the loop of wire that has one end fastened to the leads, he holding it by a wax handle, so the sparks, if the rod is electrified, will strike from the rod to the wire, and not affect him.

XI

LINNAEUS

1707-1778

Carl von Linné [Linnaeus] was born May 13, 1707, at Rashult in Smaland, Sweden. At the age of four he showed a precocious interest in plants, an interest which seriously interfered with his studies when he went to school. When his father was about to remove him, a friend urged that the boy be fitted for the profession of medicine. Linnaeus entered the university at Lund in 1727, but in the following year transferred to Upsala. In 1732, at the expense of the Academy of Sciences, he explored Lapland. Later he made pilgrimages to many of the most eminent professors of Europe, returning to Stockholm in 1738. After his marriage, in 1739, he was appointed professor at Upsala, where he continued his work in botany and established it on a rational basis. He died January 10, 1778, noted as one of the foremost botanists of his time, having discovered sex in plants and given his name to a famous botanical system of classification.

THE SEX OF PLANTS *

The organs common in general to all plants are: 1st. The root, with its capillary vessels, extracting nourishment from the ground. 2nd. The leaves, which may be called the limbs, and which, like the feet and wings of animals, are organs of motion; for being themselves shaken by the external air, they shake and exercise the plant. 3rd. The trunk, containing the medullary substance, which is nourished by the bark, and for the most part multiplied into several compound plants. 4th. The fructification, which is the true body of the plant, set at liberty by a metamorphosis, and consists only of the organs

* From the *Publications of the Linnaean Society.*

of generation; it is often defended by a calyx, and furnished with petals, by means of which it in a manner flutters in the air.

Many flowers have no calyx, as several of the lily tribe, the *Hippuris*, etc., many want the corolla, as grasses, and the plants called apetalous; but there are none more destitute of stamina and pistilla, those important organs destined to the formation of fruit. We therefore infer from experience that the stamina are the male organs of generation, and the pistilla of the female; and as many flowers are furnished with both at once, it follows that such flowers are hermaphrodites. Nor is this so wonderful, as that there should be any plants in which the different sexes are distinct individuals; for plants being immovably fixed to one spot, cannot like animals, travel in search of a mate. There exists, however, in some plants a real difference of sex. From seeds of the same mother, some individuals shall be produced, whose flowers exhibit stamina without pistilla, and may therefore properly be called male; while the rest being furnished with pistilla without stamina are therefore denominated females; and so uniformly does this take place, that no vegetable was ever found to produce female flowers without flowers furnished with stamina being produced, either on the same individual or on another plant of the same species, and *vice versa*.

As all seed vessels are destined to produce seeds, so are the stamina to bear the pollen, or fecundating powder. All seeds contain within their membranes a certain medullary substance, which swells when dipped into warm water. All pollen, likewise, contains in its membrane an elastic substance, which, although very subtle, and almost invisible, by means of warm water often explodes with great vehemence. While plants are in flower, the pollen falls from their antheræ, and is dispersed abroad, as seeds are dislodged from their situation when the fruit is ripe. At the same time that the pollen is scattered, the pistillum presents its stigma, which is then in its highest vigour, and, for a portion of the day at least, is moistened with a fine dew. The stamina either surround this stigma, or if the flowers are of the drooping kind, they are bent towards one side, so that the pollen can easily find access to the stigma, where it not only adheres by means of the dew of that part, but the moisture occasions its bursting, by which means its contents are discharged. That issued from it being mixed with the fluid of the stigma, is conveyed to rudiments of

the seed. Many evident instances of this present themselves to our notice; but I have nowhere seen it more manifest than in the Jacobean Lily (*Amaryllis formosissima*), the pistillum of which, when sufficient heat is given the plant to make it flower in perfection, is bent downwards and from its stigma issues a drop of limpid fluid, so large that one would think it in danger of falling to the ground. It is, however, gradually reabsorbed into the style about three or four o'clock and becomes invisible until about ten the next morning, when it appears again; by noon it attains its largest dimensions; and in the afternoon, by a gentle and scarcely perceptible decrease it returns to its source. If we shake the antheræ over the stigma, so that the pollen may fall on this limpid drop, we see the fluid soon after become turbid and assume a yellow color; and we perceive little rivulets, or opaque streaks running from the stigma towards the rudiments of the seed. Some time afterwards, when the drop has totally disappeared, the pollen may be observed adhering to the stigma, but of an irregular figure, having lost its original form. No one, therefore, can assent to what Morland and others have asserted, that the pollen passes into the stigma, pervades the style and enters the tender rudiments of the seed, as Leeuwenhoeck supposed his worms to enter the ova. A most evident proof of the falsehood of this opinion may be obtained from any species of *Mirabilis* (Marvel of Peru), whose pollen is so very large that it almost exceeds the style itself in thickness, and, falling on the stigma, adheres firmly to it; that organ sucking and exhausting the pollen, as a cuttle fish devours everything that comes within its grasp. One evening in the month of August, I removed all the stamina from three flowers of the *Mirabilis Longiflora*, at the same time destroying all the rest of the flowers which were expanded; I sprinkled these three flowers with the pollen of *Mirabilis Jalappa*; the seed-buds swelled, but did not ripen. Another evening I performed a similar experiment, only sprinkling the flowers with the pollen of the same species; all these flowers produced ripe seeds.

Some writers have believed that the stamina are parts of the fructification, which serve only to discharge an impure or excrementitious matter, and by no means formed for so important a work as generation. But it is very evident that these authors have not sufficiently examined the subject; for, as in many vegetables, some

flowers are furnished with stamina only, and others only with pistilla; it is altogether impossible that stamina situated at so very great a distance from the fruit, as on a different branch, or perhaps on a separate plant, should serve to convey any impurities from the embryo.

No physiologist could demonstrate, *a priori*, the necessity of the masculine fluid to the rendering the eggs of animals prolific, but experience has established it beyond a doubt. We therefore judge *a posteriori* principally, of the same effect in plants.

In the month of January, 1760, the *Antholyza Canonica* flowered in a pot in my parlour, but produced no fruit, the air of the room not being sufficiently agitated to waft the pollen to the stigma. One day, about noon, feeling the stigma very moist, I plucked off one of the antheræ, by means of a fine pair of forceps, and gently rubbed it on one part of the expanded stigmata. The spike of flowers remained eight or ten days longer; when I observed, in gathering the branch for my herbarium, that the fruit of that flower only on which the experiment had been made, had swelled to the size of a bean. I then dissected this fruit and discovered that one of the three cells contained seeds in considerable number, the other two being entirely withered.

In the month of April I sowed the seeds of hemp (*Cannabis*) in two different pots. The young plants came up so plentifully, that each pot contained thirty or forty. I placed each by the light of a window, but in different and remote apartments. The hemp grew extremely well in both pots. In one of them I permitted the male and female plants to remain together, to flower and bear fruit, which ripened in July, being macerated in water, and committed to the earth, sprung up in twelve days. From the other, however, I removed all the male plants, as soon as they were old enough for me to distinguish them from the females. The remaining females grew very well, and presented their long pistilla in great abundance, these flowers continuing a very long time, as if in expectation of their mates; while the plants in the other pot had already ripened their fruit, their pistilla having, quite in a different manner, faded as soon as the males had discharged all their pollen. It was truly a beautiful and truly admirable spectacle to see the unimpregnated females preserve their pistilla so long green and flourishing, not permitting them to begin to fade till they had been for a very con-

siderable time exposed in vain, to the access of the male pollen.

Afterwards, when these virgin plants began to decay through age, I examined all their calyces in the presence of several botanists and found them large and flourishing, although every one of the seed-buds was brown, compressed, membranaceous, and dry, not exhibiting any appearance of cotyledons or pulp. Hence I am perfectly convinced that the circumstance which authors have recorded, of the female hemp having produced seeds, although deprived of the male, could only have happened by means of pollen brought by the wind from some distant place. No experiment can be more easily performed than the above; none more satisfactory in demonstrating the generation of plants.

The *Clutia tenella* was in like manner kept growing in my window during the months of June and July. The male plant was in one pot, the female in another. The latter abounded with fruit, not one of its flowers proving abortive. I removed the two pots into different windows of the same apartment; still all the female flowers continued to become fruitful. At length I took away the male entirely, leaving the female alone, and cutting off all the flowers which it had already borne. Every day new ones appeared from the axila of every leaf; each remained eight or ten days, after which their foot stalks turning yellow, they fell barren to the ground. A botanical friend, who had amused himself with observing this phenomenon with me, persuaded me to bring, from the stove in the garden, a single male flower, which he placed over one of the female ones, then in perfection, tying a piece of red silk around its pistillum. The next day the male flower was taken away, and this single seed-bud remained, and bore fruit. Afterwards I took another male flower out of the same stove, and with a pair of slender forceps pinched off one of its antheræ, which I afterwards gently scratched with a feather, so that a very small portion of its pollen was discharged upon one of the three stigmata of a female flower, the other two stigmata being covered with paper. This fruit likewise attained its due size, and on being cut transversely, exhibited one cell filled with a large seed, and the other two empty. The rest of the flowers, being unimpregnated, faded and fell off. This experiment may be performed with as little trouble as the former.

The *Datisca cannabina* came up in my garden from seed ten years

ago, and has every year been plentifully increased by means of its perennial root. Flowers in great number have been produced by it; but, being all female, they proved abortive. Being desirous of producing male plants, I obtained more seeds from Paris. Some more plants were raised; but these likewise to my great mortification, all proved females, and bore flowers, but no fruit. In the year 1757 I received another parcel of seeds. From these I obtained a few male plants, which flowered in 1758. These were planted at a great distance from the females; and when their flowers were just ready to emit their pollen, holding a paper under them, I gently shook the spike of panicle with my finger, till the paper was almost covered with the yellow powder. I carried this to the females, which were flowering in another part of the garden, and placed it over them. The cold nights of the year in which this experiment was made, destroyed these *Datiscas*, with many other plants, much earlier than usual. Nevertheless, when I examined the flowers of those plants, which I had sprinkled with the fertilizing powder, I found the seeds of their due magnitude; while in the more remote *Datiscas*, which had not been impregnated with pollen, no traces of seeds were visible.

Several species of *Momordica*, cultivated by us, like other Indian vegetables, in close stoves, have frequently borne female flowers; which, although at first very vigorous, after a short time have constantly faded and turned yellow, without perfecting any seed, till I instructed the gardener, as soon as he observed a female flower, to gather a male one, and place it above the female. By this contrivance we are so certain of obtaining fruit that we dare pledge ourselves to make any female flowers fertile that shall be fixed on.

The *Jatropha urens* has flowered every year in my hot-house; but the female flowers coming before the males, in a week's time dropped their petals and faded before the latter were opened; from which cause no fruit has been produced, but the *germina* themselves have fallen off. We have therefore never had any fruit of the *Jatropha* till the year 1752, when the male flowers were in vigour on a tall tree, at the same time that the females began to appear on a small *Jatropha* which was growing in a garden-pot. I placed this pot under the other tree, by which means the female flowers bore seeds, which grew on being sown. I have frequently amused myself with taking the male flowers from one plant, and scattering them over the female

flowers of another, and have always found the seeds of the latter impregnated by it.

Two years ago I placed a piece of paper under some of these male flowers and afterwards folded up the pollen which had fallen upon it, preserving it so folded up, if I remember right, four or six weeks, at the end of which time another branch of the same *Jatropha* was in flower. I then took the pollen, which I had so long preserved in paper, and strewed it over three female flowers, the only ones at that time expanded. The three females proved fruitful, while all the rest, which grew in the same bunch, fell off abortive.

The interior petals of the *Ornithogalum*, commonly but improperly called *Canadense*, cohere so closely together that they only just admit the air to the germen and will scarcely permit the pollen of another flower to pass; this plant produced every day new flowers and fruit, the fructification never failing in any instance; I therefore, with the utmost care, extracted the antheræ from one of the flowers with a hooked needle, and as I hoped, this single flower proved barren. This experiment was repeated about a week after with the same success.

I removed all of the antheræ out of a flower of *Chelidonium corniculatum* (scarlet-horned poppy), which was growing in a remote part of the garden, upon the first opening of its petals, and stripped off all the rest of the flowers; another day I treated another flower of the same plant in a similar manner, but sprinkled the pistillum of this with the pollen borrowed from another plant of the same species; the result was, that the first flower produced no fruit, but the second afforded very perfect seed. My design in this experiment was to prove that the mere removal of the antheræ from a flower is not in itself sufficient to render the germen abortive.

Having the *Nicotiana fruticosa* growing in a garden-pot, and producing plenty of flowers and seed, I extracted the antheræ from the newly expanded flowers before they had burst, at the same time cutting away all the other flowers; this germen produced no fruit, nor did it even swell.

I removed an urn, in which the *Asphodelus fistulosus* was growing, to one corner of the garden, and from one of the flowers which had lately opened, I extracted its antheræ; this caused the impregna-

tion to fail. Another day I treated another flower in the same manner; but, bringing a flower from a plant in a different part of the garden, with which I sprinkled the pistillum of the mutilated one, its germen became by that means fruitful.

Ixia chinensis, flowering in my stove, the windows of which were shut, all its flowers proved abortive. I therefore took one of its antheræ in a pair of pincers, and with them sprinkled the stigmata of two flowers, and the next day one stigma only of a third flower; the seed-buds of these flowers remained, grew to a large size and bore seed, the fruit of the third, however, contained ripe seed only in one of its cells.

To relate more experiments would only be to fatigue the reader unnecessarily. All nature proclaims the truth I have endeavored to inculcate, and every flower bears witness to it. Any person may make the experiment for himself with any plant he pleases, only taking care to place the pot in which it is growing, in the window of a room sufficiently out of reach of other flowers; and I will venture to promise him that he will obtain no perfect fruit unless pollen has access to the pistillum.

Logan's experiments on the Mays are perfectly satisfactory, and manifestly show that the pollen does not enter the style, or arrive at the germen, but that it is exhausted by the genital fluid of the pistillum. And as in animals no conception can take place, unless the genital fluid of the female be discharged at the same moment as the impregnating liquor of the male; so in plants, generation fails, unless the stigma be moist with prolific dew.

Husbandmen know, by long experience, that if rain falls while rye is in flower, by coagulating the pollen of its antheræ, it occasions the emptiness of many husks in the ear.

Gardeners remark the same thing every year in fruit trees. Their blossoms produce no fruit if they have unfortunately been exposed to long-continued rains.

Aquatic plants rise above the water at the time of flowering, and afterwards again subside, for no other reason, than that the pollen may safely reach the stigma.

The white water-lily (*Nymphaea alba*) raises itself every morning out of the water and opens its flowers, so that by noon at least three inches of its flower-stalk may be seen above the surface. In the

evening it is closely shut up, and withdrawn again; for about four o'clock in the afternoon the flower closes, and remains all night under water; which was observed full two thousand years since, even as long ago as the time of Theophrastus, who has described this circumstance in the *Nymphaea Lotus*, a plant so much resembling our white water-lily that they are only distinguished from each other by the leaves of the Lotus being indented. Theophrastus gives the following account of this vegetable, in his *History of Plants*, book IV., chap. 10: "It is said to withdraw its flowers into the Euphrates, which continue to descend till midnight, to so great a depth that at daybreak they are out of reach of the hand; after which it rises again, and in the course of the morning appears above the water, and expands its flowers, rising higher and higher, till it is a considerable height above the surface." The very same thing may be observed in the *Nymphaea alba*.

Many flowers close themselves in the evening and before rain, lest the pollen should be coagulated; but after the discharge of the pollen they always remain open. Such of them as do not shut up, incline their flowers downward in those circumstances, and several flowers, which come forth in the moisture of spring, droop perpetually. The manner in which the Parnassia and Saxifrage move their antheræ to the stigma is well known. The common Rue, a plant everywhere to be met with, moves one of its antheræ every day to the stigma, till all of them in their turns have deposited their pollen there.

The Neapolitan star flower (*Ornithogalum nutans*) has six broad stamina, which stand close together in the form of a bell, the three external ones being but half the length of the others; so that it seems impossible for their antheræ ever to convey their pollen to the stigma; but nature, by an admirable contrivance, bends the summits of these external stamina inwards between the other filaments, so that they are enabled to accomplish their purpose.

The Plaintain tree (*Musa*) bears two kinds of hermaphrodite flowers; some have imperfect antheræ, others only the rudiments of stigmata; as the last mentioned kind appear after the others, they cannot impregnate them, consequently no seeds are produced in our gardens, and scarcely ever on the plants cultivated in India. An event happened this year, which I have long wished for; two plaintain-

trees flowering with me so fortunately that one of them brought forth its first female blossoms at the time that male ones began to appear on the other. I eagerly ran to collect antheræ from the first plant, in order to scatter them over the newly-expanded females, in hopes of obtaining seed from them, which no botanist has yet been able to do. But when I came to examine the antheræ I found even the largest of them absolutely empty and void of pollen, consequently unfit for impregnating the females; the seeds of this plant, therefore, can never be perfected in our gardens. I do not doubt, however, that real male plants of this species may be found in its native country, bearing flowers without fruit, which the gardeners have neglected; while the females in this country produce imperfect fruit, without seeds, like the female fig; and, like that tree, are increased easily by suckers. The fruit, therefore, of the plaintain-tree scarcely attains anything like its due size, the larger seed-buds only ripening, without containing anything in them.

The day would sooner fail me than examples. A female date-bearing palm flowered many years at Berlin, without producing any seeds. But the Berlin people taking care to have some of the blossoms of the male tree, which was then flowering at Leipsic, sent them by the post, they obtained fruit by that means; and some dates, the offspring of this impregnation, being planted in my garden, sprung up, and to this day continue to grow vigorously. Kœmpfer formerly told us how necessary it was found by the oriental people, who live upon the produce of palm-trees, and are the true Lotophagi, to plant some male trees among the females, if they hoped for any fruit; hence, it is the practice of those who make war in that part of the world to cut down all the male palms, that a famine may afflict their proprietors; sometimes even the inhabitants themselves destroy the male trees, when they dread an invasion, that their enemies may find no sustenance in the country.

Leaving these instances, and innumerable others, which are so well known to botanists that they would by no means bear the appearance of novelty, and can only be doubted by those persons who neither have observed nature, nor will they take the trouble to study her, I pass to a fresh subject, concerning which much new light is wanted; I mean hybrid, or mule vegetables, the existence and origin of which we shall now consider.

I shall enumerate three or four real mule plants, to whose origin I have been an eye-witness.

1. *Veronica spuria*, described in *Amœnitates Acad.* vol. III. p. 35, came from the impregnation of *Veronic maratima* by *Verbena officinalis*; it is easily propagated by cuttings, and agrees perfectly with its mother in fructification, and with its father in leaves.

2. *Delphinium hybridum*, sprung up in a part of the garden where *Delphinium clatum* and *Aconitum Napellus* grew together; it resembles its mother as much in its internal parts, that is, in fructification as it does its father (the *Aconitum*) in outward structure, or leaves; and, owing its origin to plants so nearly allied to each other, it propagates itself by seed; some of which I now send with this Dissertation.

3. *Hieracium Taraxici*, gathered in 1753 upon our mountains by Dr. Solander, in its thick, brown, woolly calyx; in its stem being hairy towards the top, and in its bractæ, as well as in every part of its fructification, resembles so perfectly its mother, *Hieracium alpinum*, that an inexperienced person might mistake one for the other; but in the smoothness of its leaves, in their indentations and whole structure, it so manifestly agrees with its father, *Leontodon Taraxacum* (Dandelion), that there can be no doubt of its origin.

4. *Tragopogon hybridum* attracted my notice the autumn before last, in a part of the garden where I had planted *Tragopogon pratense*, and *Tragopogon porrifolium*; but winter coming on, destroyed its seeds. Last year, while the *Tragopogon pratense* was in flower I rubbed off its pollen early in the morning, and about eight o'clock sprinkled its stigmata with some pollen of the *Tragopogon porrifolium*, marking the calyces by tying a thread round them. I afterwards gathered the seeds when ripe, and sowed them that autumn in another place; they grew, and produced this year, 1759, purple flowers yellow at the base, seeds of which I now send. I doubt whether any experiment demonstrates the generation of plants more certainly than this.

There can be no doubt that these are all new species produced by hybrid generation. And hence we learn, that a mule offspring is the exact image of its mother in its medullary substance, internal nature, or fructification, but resembles its father in leaves. This is a foundation upon which naturalists may build much. For it seems probable

that many plants, which now appear different species of the same *genus*, may in the beginning have been but one plant, having arisen merely from hybrid generation. Many of those *Geraniums* which grow at the Cape of Good Hope, and have never been found wild anywhere but in the south parts of Africa, and which, as they are distinguished from all other *Geraniums* by their single-leaved calyx, many-flowered foot-stalk, irregular corolla, seven fertile stamina, and three mutilated ones, and by their naked seeds furnished with downy awns; so they agree together in all these characters, although very various in their roots, stems and leaves; these *Geraniums*, I say, would almost induce a botanist to believe that the species of one *genus* in vegetables are only so many different plants as there have been different associations with the flowers of one species, and consequently a *genus* is nothing else than a number of plants sprung from the same mother by different fathers. But whether all these species be the offspring of time; whether, in the beginning of all things, the Creator limited the number of future species, I dare not presume to determine. I am, however, convinced this mode of multiplying plants does not interfere with the system or general scheme of nature; as I daily observe that insects, which live upon one species of a particular *genus*, are contented with another of the same *genus*.

A person who has once seen the *Achyranthes aspera*, and remarked its spike, the parts of its flower, its small and peculiarly formed nectaria, as well as its calyces bent backwards as the fruit ripens, would think it very easy at any time to distinguish these flowers from all others in the universe; but when he finds the flowers of *Achyranthes indica* agreeing with them even in their minutest parts, and at the same time observes the large, thick, obtuse, undulated leaves of the last-mentioned plant, he will think he sees *Achyranthes aspera* masked in the foliage of *Xanthium strumarium*. But I forbear to mention any more instances.

Here is a new employment for botanists, to attempt the production of new species of vegetables by scattering the pollen of various plants over various widowed females. And if these remarks should meet with a favourable reception, I shall be the more induced to dedicate what remains of my life to such experiments, which recommend themselves by being at the same time agreeable and useful. I am persuaded by many considerations that those numerous and most

valuable varieties of plants which are used for culinary purposes, have been produced in this manner, as the several kinds of cabbages, lettuces, etc.; and I apprehend this is the reason of their not being changed by a difference of soil. Hence I cannot give my assent to the opinion of those who imagine all varieties to have been occasioned by change of soil; for, if this were the case, the plants would return to their original form, if removed again to their original situation.

XII

JOSEPH BLACK

1728-1799

Joseph Black, born in 1728 at Bordeaux, France, was educated in Belfast and at the University of Glasgow. Before he took his M.D. degree he showed that alkalies were formed, not by their absorbing "phlogiston," but by their having carbonic acid gas, or "fixed air," as a component. In 1753 he was appointed lecturer on chemistry at Glasgow, and in 1776 became professor of chemistry at Edinburgh. In 1763 he announced his discovery of latent heat, a principle which has been of great practical value. He died in Edinburgh, December 6, 1799.

THE DISCOVERY OF CARBONIC ACID GAS *

Hoffman, in one of his observations, gives the history of a powder called *Magnesia Alba*, which has been long used, and esteemed as a mild and tasteless purgative; but the method of preparing it was not generally known before he made it public.

It was originally obtained from a liquor called the *Mother of nitre*, which is produced in the following manner:

Salt-petre is separated from the brine which first affords it, or from the water with which it is washed out of nitrous earths, by the process commonly used in crystallizing salts. In this process, the brine is gradually diminished, and at length reduced to a small quantity of an unctuous bitter saline liquor, affording no more salt-petre by evaporation, but, if urged with a brisk fire, drying up into a confused mass, which attracts water strongly, and becomes fluid again when exposed to the open air.

To this liquor the workmen have given the name of the *Mother of*

* From *Experiments upon Magnesia, Quicklime, and some other Alkaline Substances* (1775).

nitre; and Hoffman, finding it composed of the magnesia united to an acid, obtained a separation of these, either by exposing the compound to a strong fire, in which the acid was dissipated, and the magnesia remained behind, or by the addition of an alkali, which attracted the acid to itself: and this last method he recommends as the best. He likewise makes an inquiry into the nature and virtues of the powder thus prepared; and observes, that it is an absorbent earth, which joins readily with all acids, and must necessarily destroy any acidity it meets in the stomach; but that its purgative power is uncertain, for sometimes it has not the least effect of that kind. As it is a mere insipid earth, he rationally concludes it to be a purgative only when converted into a sort of neutral salt by an acid in the stomach, and that its effect is therefore proportional to the quantity of this acid.

Although magnesia appears from this history of it, to be a very innocent medicine; yet, having observed that some hypochondriacs, who used it frequently, were subject to flatulencies and spasms, he seems to have suspected it of some noxious quality. The circumstances, however, which gave rise to his suspicion, may very possibly have proceeded from the imprudence of his patients; who, trusting too much to magnesia (which is properly a palliative in that disease) and neglecting the assistance of other remedies, allowed their disorder to increase upon them. It may, indeed, be alleged that magnesia, as a purgative, is not the most eligible medicine for such constitutions, as they agree best with those that strengthen, stimulate, and warm; which the saline purges, commonly used, are not observed to do. But there seems at last to be no objection to its use, when children are troubled with an acid in their stomach: for, gentle purging, in this case, is very proper; and it is often more conveniently procured by means of magnesia, than of any other medicine, on account of its being entirely insipid.

The above-mentioned Author, observing, some time after, that a bitter saline liquor, similar to that obtained from the brine of salt-petre, was likewise produced by the evaporation of those waters which contain common salt, had the curiosity to try if this would also yield a magnesia. The experiment succeeded: And he thus found out another process for obtaining this powder; and at the same time

assured himself, by experiments, that the product from both was exactly the same.

My curiosity led me, some time ago, to inquire more particularly into the nature of magnesia, and especially to compare its properties with those of the other absorbent earths, of which there plainly appeared to me to be very different kinds, although commonly confounded together under one name. I was indeed led to this examination of the absorbent earths, partly by the hope of discovering a new sort of lime and lime-water, which might possibly be a more powerful solvent of the stone, than that commonly used; but was disappointed in my expectations.

I have had no opportunity of seeing Hoffman's first magnesia, or the liquor from which it is prepared, and have therefore been obliged to make my experiments upon the second.

In order to prepare it, I at first employed the bitter saline liquor called *bittern*, which remains in the pans after the evaporation of seawater. But as that liquor is not always easily procured, I afterwards made use of a salt called Epsom salt, which is separated from the bittern by crystallization, and is evidently composed of magnesia and the vitriolic acid.

There is likewise a spurious kind of Glauber salt, which yields plenty of magnesia, and seems to be no other than Epsom salt, of seawater reduced to crystals of a larger size. And common salt also affords a small quantity of this powder; because, being separated from the bittern by one hasty crystallization only, it necessarily contains a portion of that liquor.

Those who would prepare a magnesia from Epsom salt, may use the following process:

Dissolve equal quantities of Epsom salt, and of pearl ashes, separately, in a sufficient quantity of water; purify each solution from its dregs, and mix them accurately together by violent agitation. Then make them just to boil over a brisk fire.

Add now to the mixture, three or four times its quantity of hot water; after a little agitation, allow the magnesia to settle to the bottom, and decant off as much of the water as possible. Pour on the same quantity of cold water; and, after settling, decant it off in the same manner. Repeat this washing with the cold water ten or twelve

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times, or even oftener, if the magnesia be required perfectly pure for chemical experiments.

When it is sufficiently washed, the water may be strained and squeezed from it in a linen cloth ; for very little of the magnesia passes through.

The alkali in the mixture, uniting with the acid, separates it from the magnesia ; which, not being of itself soluble in water, must consequently appear immediately under a solid form. But the powder which thus appears is not entirely magnesia ; part of it is the neutral salt formed from the union of the acid and alkali. This neutral salt is found, upon examination, to agree in all respects with vitriolated tartar, and requires a large quantity of hot water to dissolve it. As much of it is therefore dissolved as the water can take up ; the rest is dispersed through the mixture, in the form of a powder. Hence the necessity of washing the magnesia with so much trouble ; for the first effusion of hot water is intended to dissolve the whole of the salt, and the subsequent additions of cold water to wash away this solution.

The caution given, of boiling the mixture, is not unnecessary : if it be neglected, the whole of the magnesia is not accurately separated at once ; and, by allowing it to rest for some time, that powder concretes into minute grains, which, when viewed with the microscope, appear to be assemblages of needles diverging from a point. This happens more especially when the solutions of the Epsom salt, and of the alkali, are diluted with too much water before they are mixed together. Thus, if a dram of Epsom salt, and of salt of tartar, be dissolved each in four ounces of water, and be mixed, and then allowed to rest three or four days, the whole of the magnesia will be formed into these grains. Or, if we filtrate the mixture soon after it is made, and heat the clear liquor which passes through, it will become turbid, and deposit a magnesia.

An ounce of magnesia was exposed in a crucible, for about an hour, to such a heat as is sufficient to melt copper. When taken out, it weighed three drams and one scruple, or had lost $\frac{7}{12}$ of its former weight.

I repeated, with the magnesia prepared in this manner, most of

those experiments I had already made upon it before calcination, and the result was as follows:—

It dissolves in all the acids, and with these composes salts exactly similar to those described in the first set of experiments: But, what is particularly to be remarked, it is dissolved without any the least degree of effervescence.

It slowly precipitates the corrosive sublimate of mercury, in the form of a black powder.

It separates the volatile alkali in salt-ammoniac from the acid, when it is mixed with a warm solution of that salt. But it does not separate an acid from a calcareous earth, nor does it introduce the least change upon lime-water.

Lastly, when a dram of it is digested with an ounce of water in a bottle for some hours, it does not make any the least change in the water. The magnesia, when dried, is found to have gained ten grains; but it neither effervesces with acids, nor does it sensibly affect lime-water.

Observing magnesia to lose such a remarkable proportion of its weight in the fire, my next attempts were directed to the investigation of this volatile part; and, among other experiments, the following seemed to throw some light upon it:—

Three ounces of magnesia were distilled in a glass retort and receiver, the fire being gradually increased until the magnesia was obscurely red hot. When all was cool, I found only five drams of a whitish water in the receiver, which had a faint smell of the spirit of hartshorn, gave a green colour to the juice of violets, and rendered the solutions of corrosive sublimate, and of silver, very slightly turbid. But it did not sensibly effervesce with acids.

The magnesia, when taken out of the retort, weighed an ounce, three drams, and thirty grains, or had lost more than half of its weight. It still effervesced pretty briskly with acids, though not so strongly as before this operation.

The fire should have been raised here to the degree requisite for the perfect calcination of magnesia. But, even from this imperfect experiment, it is evident, that, of the volatile parts contained in that powder, a small proportion only is water; the rest cannot, it seems, be retained in vessels, under a visible form. Chemists have often observed in their distillations that part of a body has vanished from

their senses notwithstanding the utmost care to retain it; and they have always found, upon further inquiry, that subtle part to be air, which having been imprisoned in the body, under a solid form, was set free, and rendered fluid and elastic by the fire. We may therefore safely conclude, that the volatile matter lost in the calcination of magnesia, is mostly air; and hence the calcined magnesia does not emit air, or make an effervescence when mixed with acids.

The water, from its properties, seems to contain a small portion of volatile alkali, which was probably formed from the earth, air and water, from some of these combined together; and perhaps also from a small quantity of inflammable matter, which adhered accidentally to the magnesia. Whenever chemists meet with this salt, they are inclined to ascribe its origin to some animal or putrid vegetable substance; and this they have always done, when they obtained it from the calcareous earths, all of which afford a small quantity of it. There is, however, no doubt, that it can sometimes be produced independently of any such mixture, since many fresh vegetables, and tartar, afford a considerable quantity of it. And how can it, in the present instance, be supposed, that any animal or vegetable matter adhered to the magnesia, while it was dissolved by an acid, separated from this by an alkali, and washed with so much water?

Two drams of magnesia were calcined in a crucible, in the manner described above, and thus reduced to two scruples and twelve grains. This calcined magnesia was dissolved in a sufficient quantity of spirit of vitriol, and then again separated from the acid by the addition of an alkali, of which a large quantity is necessary for this purpose. The magnesia being very well washed and dried, weighed one dram and fifty grains. It effervesced violently, or emitted a large quantity of air, when thrown into acids; formed a red powder, when mixed with a solution of sublimate; separated the calcareous earths from an acid, and sweetened lime-water; and had thus recovered all those properties which it had but just now lost by calcination. Nor had it only recovered its original properties, but acquired besides an addition of weight, nearly equal to what had been lost in the fire; and as it is found to effervesce with acids, part of the addition must certainly be air.

This air seems to have been furnished by the alkali, from which it was separated by the acid; for Dr. Hales has clearly proved, that

alkaline salts contain a large quantity of fixed air, which they emit in great abundance when joined to a pure acid. In the present case, the alkali is really joined to an acid, but without any visible emission of air; and yet the air is not retained in it; for the neutral salt, into which it is converted, is the same in quantity, and in every other respect, as if the acid employed had not been previously saturated with magnesia, but offered to the alkali in its pure state, and had driven the air out of it in their conflict. It seems therefore evident, that the air was forced from the alkali by the acid, and lodged itself in the magnesia.

These considerations led me to try a few experiments, whereby I might know what quantity of air is expelled from an alkali, or from magnesia, by acids.

Two drams of a pure fixed alkaline salt, and an ounce of water, were put into a Florentine flask, which, together with its contents, weighed two ounces and two drams. Some oil of vitriol diluted with water was dropped in, until the salt was exactly saturated; which it was found to be, when two drams, two scruples and three grains of this acid had been added. The phial with its contents now weighed two ounces, four drams and fifteen grains. One scruple, therefore, and eight grains, were lost during the ebullition; of which a trifling portion may be water, or something of the same kind; the rest is air.



XIII

JOSEPH PRIESTLEY

1733-1804

Joseph Priestley, born in Yorkshire, England, March 13, 1733, was a Unitarian minister. In 1774 he discovered oxygen, which he called "dephlogisticated air." Because of his liberal political ideas he was persecuted by his countrymen, and in 1794 emigrated to Northumberland, Pennsylvania, where he lived until his death, February 6, 1804.

THE DISCOVERY OF OXYGEN *

Presently, after my return from abroad, I went to work upon the *mercurius calcinatus*, which I had procured from Mr. Cadet; and, with a very moderate degree of heat, I got from about one-fourth of an ounce of it, an ounce-measure of air, which I observed to be not readily imbibed, either by the substance itself from which it had been expelled (for I suffered them to continue a long time together before I transferred the air to any other place) or by water, in which I suffered this air to stand a considerable time before I made any experiment upon it.

In this air, as I had expected, a candle burned with a vivid flame; but what I observed new at this time (November 19), and which surprised me no less than the fact I had discovered before, was, that, whereas a few moments agitation in water will deprive the modified nitrous air of its property of admitting a candle to burn in it; yet, after more than ten times as much agitation as would be sufficient to produce this alteration in the nitrous air, no sensible change was produced in this. A candle still burned in it with a strong flame; and it

* From *Experiments and Observations on Different Kinds of Air*, Vol. II, (1775).

did not, in the least, diminish common air, which I have observed that nitrous air, in this state, in some measure does.

But I was much more surprised, when, after two days, in which this air had continued in contact with water (by which it was diminished about one-twentieth of its bulk) I agitated it violently in water about five minutes, and found that a candle still burned in it as well as in common air. The same degree of agitation would have made phlogisticated nitrous air fit for respiration indeed, but it would certainly have extinguished a candle.

These facts fully convinced me, that there must be a very material difference between the constitution of air from *mercurius calcinatus*, and that of phlogisticated nitrous air, notwithstanding their resemblance in some particulars. But though I did not doubt that the air from *mercurius calcinatus* was fit for respiration, after being agitated in water, as every kind of air without exception, on which I have tried the experiment, had been, I still did not suspect that it was respirable in the first instance; so far was I from having any idea of this air being, what it really was, much superior, in this respect, to the air of the atmosphere.

In this ignorance of the real nature of this kind of air, I continued from this time (November) to the 1st of March following; having, in the meantime, been intent upon my experiments on the vitriolic acid air above recited, and the various modifications of air produced by spirit of nitre, an account of which will follow. But in the course of this month, I not only ascertained the nature of this kind of air, though very gradually, but was led to it by the complete discovery of the constitution of the air we breathe.

Till this 1st of March, 1775, I had so little suspicion of the air from *mercurius calcinatus*, &c., being wholesome, that I had not even thought of applying it to the test of nitrous air; but thinking (as my reader must imagine I frequently must have done) on the candle burning in it after long agitation in water, it occurred to me at last to make the experiment; and putting one measure of nitrous air to two measures of this air, I found, not only that it was diminished, but that it was diminished quite as much as common air, and that the redness of the mixture was likewise equal to that of a similar mixture of nitrous and common air.

After this I had no doubt but that the air from *mercurius calcinatus*

was fit for respiration, and that it had all the other properties of genuine common air. But I did not take notice of what I might have observed, if I had not been so fully possessed by the notion of there being no air better than common air, that the redness was really deeper, and the diminution something greater than common air would have admitted.

Moreover, this advance in the way of truth, in reality, threw me back into error, making me give up the hypothesis I had first formed, viz. that the *mercurius calcinatus* had extracted spirit of nitre from the air; for I now concluded, that all the constituent parts of the air were equally, and in their proper proportion, imbibed in the preparation of this substance, and also in the process of making red lead. For at the same time that I made the above mentioned experiment on the air from *mercurius calcinatus*, I likewise observed that the air which I had extracted from red lead, after the fixed air was washed out of it, was of the same nature, being diminished by nitrous air like common air: but, at the same time, I was puzzled to find that air from the red precipitate was diminished in the same manner, though the process for making this substance is quite different from that of making the two others. But to this circumstance I happened not to give much attention.

I wish my reader be not quite tired with the frequent repetition of the word surprise, and others of similar import; but I must go on in that style a little longer. For the next day I was more surprised than ever I had been before, with finding that, after the above-mentioned mixture of nitrous air and the air from *mercurius calcinatus*, had stood all night, (in which time the whole diminution must have taken place; and, consequently, had it been common air, it must have been made perfectly noxious, and entirely unfit for respiration or inflammation) a candle burned in it, and even better than in common air.

I cannot, at this distance of time, recollect what it was that I had in view in making this experiment; but I know I had no expectation of the real issue of it. Having acquired a considerable degree of readiness in making experiments of this kind, a very slight and evanescent motive would be sufficient to induce me to do it. If, however, I had not happened, for some other purpose, to have had a lighted candle before me I should probably never have made the trial; and the whole

train of my future experiments relating to this kind of air might have been prevented.

Still, however, having no conception of the real cause of this phenomenon, I considered it as something very extraordinary; but as a property that was peculiar to air that was extracted from these substances, and adventitious; and I always spoke of the air to my acquaintance as being substantially the same thing with common air.

I particularly remember my telling Dr. Price, that I was myself perfectly satisfied of its being common air, as it appeared to be so by the test of nitrous air; though, for the satisfaction of others, I wanted a mouse to make the proof quite complete.

On the 8th of this month I procured a mouse, and put it into a glass vessel, containing two ounce-measures of the air from *mercuris calcinatus*. Had it been common air, a full-grown mouse, as this was, would have lived in it about a quarter of an hour. In this air, however, my mouse lived a full half hour; and though it was taken out seemingly dead, it appeared to have been only exceedingly chilled; for, upon being held to fire, it presently revived, and appeared not to have received any harm from the experiment.

By this I was confirmed in my conclusion, that the air extracted from *mercurius calcinatus*, &c., was, at least, as good as common air; but I did not certainly conclude that it was any better; because, though one mouse would live only a quarter of an hour in a given quantity of air, I knew it was not impossible but that another mouse might have lived in it half an hour; so little accuracy is there in this method of ascertaining the goodness of air; and indeed I have never had recourse to it for my own satisfaction, since the discovery of that most ready, accurate, and elegant test that nitrous air furnishes. But in this case I had a view to publishing the most generally satisfactory account of my experiments that the nature of the thing would admit of.

This experiment with the mouse, when I had reflected upon it some time, gave me so much suspicion that the air into which I had put it was better than common air, that I was induced, the day after, to apply the test of nitrous air to a small part of that very quantity of air which the mouse had breathed so long; so that, had it been common air, I was satisfied it must have been very nearly, if not altogether, as

noxious as possible, so as not to be affected by nitrous air; when, to my surprise again, I found that though it had been breathed so long, it was still better than common air. For after mixing it with nitrous air, in the usual proportion of two to one, it was diminished in the proportion of four and one-half to three and one-half; that is, the nitrous air had made it two-ninths less than before, and this in a very short space of time; whereas I had never found that, in the longest time, any common air was reduced more than one-fifth of its bulk by any proportion of nitrous air, nor more than one-fourth by any phlogistic process whatever. Thinking of this extraordinary fact upon my pillow, the next morning I put another measure of nitrous air to the same mixture, and, to my utter astonishment, found that it was farther diminished to almost one-half of its original quantity. I then put a third measure to it; but this did not diminish it any farther; but, however, left it one measure less than it was even after the mouse had been taken out of it.

Being now fully satisfied that this air, even after the mouse had breathed it half an hour, was much better than common air; and having a quantity of it still left, sufficient for the experiment, viz. an ounce-measure and a half, I put the mouse into it; when I observed that it seemed to feel no shock upon being put into it, evident signs of which would have been visible, if the air had not been very wholesome; but that it remained perfectly at its ease another full half hour, when I took it out quite lively and vigorous. Measuring the air the next day, I found it to be reduced from one and one-half to two-thirds of an ounce-measure. And after this, if I remember well (for in my register of the day I only find it noted, that it was considerably diminished by nitrous air), it was nearly as good as common air. It was evident, indeed, from the mouse having been taken out quite vigorous, that the air could not have been rendered very noxious.

For my farther satisfaction I procured another mouse, and putting it into less than two ounce-measures of air extracted from *mercurius calcinatus* and air from red precipitate (which, having found them to be of the same quality, I had mixed together) it lived three-quarters of an hour. But not having had the precaution to set the vessel in a warm place, I suspect that the mouse died of cold. However, as it had lived three times as long as it could probably have lived in the same quantity of common air, and I did not expect much accuracy

from this kind of a test, I did not think it necessary to make any more experiments with mice.

Being now fully satisfied of the superior goodness of this kind of air, I proceeded to measure that degree of purity, with as much accuracy as I could, by the test of nitrous air; and I began with putting one measure of nitrous air to two measures of this air, as if I had been examining common air; and now I observed that the diminution was evidently greater than common air would have suffered by the same treatment. A second measure of nitrous air reduced it to two-thirds of its original quantity, and a third measure to one-half. Suspecting that the diminution could not proceed much farther, I then added only half a measure of nitrous air, by which it was diminished still more; but not much, and another half-measure made it more than half of its original quantity; so that, in this case, two measures of this air took more than two measures of nitrous air, and yet remained less than half of what it was. Five measures brought it pretty exactly to its original dimensions.

At the same time, air from the red precipitate was diminished in the same proportion as that from *mercurius calcinatus*, five measures of nitrous air being received by two measures of this without any increase of dimensions. Now as common air takes about one-half of its bulk of nitrous air, before it begins to receive any addition to its dimensions from more nitrous air, and this air took more than four half-measures before it ceased to be diminished by more nitrous air, and even five half-measures made no addition to its original dimensions, I conclude that it was between four and five times as good as common air. It will be seen that I have since procured air better than this, even between five and six times as good as the best common air that I have ever met with.

XIV

HENRY CAVENDISH

1731-1810

Henry Cavendish, the discoverer of hydrogen, was born of English parents in Nice, October 10, 1731. He studied at Cambridge University, England, and in 1760 joined the Royal Society, devoting his great fortune to the advancement of science. He discovered hydrogen in 1766, and later, using Priestley's discovery of oxygen, found that the two gases combined under certain physical conditions to produce water. Besides his studies in chemistry, he made some interesting discoveries in physics. In 1783 he proposed the theory that heat was a motion rather than a substance; and in 1798 he computed the density of the earth to be about five and a half times that of water. He died at Clapham, February 24, 1810.

THE COMBINATION OF HYDROGEN AND OXYGEN INTO WATER *

In Dr. Priestley's last volume of experiments is related an experiment of Mr. Warltire's, in which it is said that, on firing a mixture of common and inflammable air by electricity in a close copper vessel holding about three pints, a loss of weight was always perceived, on an average about two grains, though the vessel was stopped in such a manner that no air could escape by the explosion. It is also related, that on repeating the experiment in glass vessels, the inside of the glass, though clean and dry before, immediately became dewy; which confirmed an opinion he had long entertained, that common air deposits its moisture by phlogistication. As the latter experiment seemed likely to throw great light on the subject I had in view, I thought it well worth examining more closely. The first experiment also, if there was no mistake in it, would be very extraordinary and

* From *Experiments with Airs*—*Transactions of Royal Society of London* (1784).

curious; but it did not succeed with me; for though the vessel I used held more than Mr. Warltire's, namely, 24,000 grains of water, and though the experiment was repeated several times with different proportions of common and inflammable air, I could never perceive a loss of weight of more than one-fifth of a grain, and commonly none at all. It must be observed, however, that though there were some of the experiments in which it seemed to diminish a little in weight, there were none in which it increased.

In all the experiments, the inside of the glass globe became dewy, as observed by Mr. Warltire; but not the least sooty matter could be perceived. Care was taken in all of them to find how much the air was diminished by the explosion, and to observe its test. The result is as follows, the bulk of the inflammable air being expressed in decimals of the common air:

Common Air	Inflammable Air	Diminution	Air Remaining after the Explosion	Test of this Air in the First Method	Standard
I	1.241	.686	1.555	.055	.0
	1.955	.642	1.423	.063	.0
	.706	.647	1.059	.066	.0
	.423	.612	.811	.097	.03
	.331	.476	.855	.339	.27
	.206	.294	.912	.648	.58

In these experiments the inflammable air was procured from zinc, as it was in all my experiments, except where otherwise expressed: but I made two more experiments, to try whether there was any difference between the air from zinc and that from iron, the quantity of inflammable air being the same in both, namely, 0.331 of the common; but I could not find any difference to be depended on between the two kinds of air, either in the diminution which they suffered by the explosion, or the test of the burnt air.

From the fourth experiment it appears, that 423 measures of inflammable air are nearly sufficient to phlogisticate completely 1000 of common air; and that the bulk of the remaining air after the explosion is then very little more than four-fifths of the common air employed; so that as common air cannot be reduced to a much less bulk than that by any method of phlogistication, we may safely con-

clude, that when they are mixed in this proportion, and exploded, almost all the inflammable air, and about one-fifth part of the common air, lose their elasticity, and are condensed into the dew which lines the glass.

The better to examine the nature of this dew, 500,000 grain measures of inflammable air were burnt with about two and one-half times the quantity of common air, and the burnt air made to pass through a glass cylinder eight feet long and three-quarters of an inch in diameter, in order to deposit the dew. The two airs were conveyed slowly into this cylinder by separate copper pipes, passing through a brass plate which stopped up the end of the cylinder; and as neither inflammable nor common air can burn by themselves, there was no danger of the flame spreading into the magazines from which they were conveyed. Each of these magazines consisted of a large tin vessel, inverted into another vessel just big enough to receive it. The inner vessel communicated with the copper pipe, and the air was forced out of it by pouring water into the outer vessel; and in order that the quantity of common air expelled should be two and one-half times that of the inflammable, the water was let into the outer vessels by two holes in the bottom of the same tin pan, the hole which conveyed the water into that vessel in which the common air was confined being two and one-half times as big as the other.

In trying the experiment, the magazines being first filled with their respective airs, the glass cylinder was taken off, and water let, by the two holes, into the outer vessel, till the airs began to issue from the ends of the copper pipes; they were then set on fire by a candle, and the cylinder put on again in its place. By this means upwards of 135 grains of water were condensed in the cylinder, which had no taste nor smell, and which left no sensible sediment when evaporated to dryness; neither did it yield any pungent smell during evaporation; in short, it seemed pure water.

In my first experiment, the cylinder near that part where the air was fired was a little tinged with sooty matter, but very slightly so; and that little seemed to proceed from the putty with which the apparatus was luted, and which was heated by the flame; for in another experiment, in which it is contrived so that the luting should not be much heated, scarce any sooty tinge could be perceived.

By the experiments with the globe it appeared, that when inflam-

mable and common air are exploded in a proper proportion, almost all the inflammable air, and nearly one-fifth of the common air, lose their elasticity, and are condensed into dew. And by this experiment it appears, that this dew is plain water, and consequently that almost all the inflammable air and about one-fifth of the common air, are turned into pure water.

In order to examine the nature of the matter condensed on firing a mixture of dephlogisticated and inflammable air, I took a glass globe holding 8,800 grain measures, furnished with a brass cock and an apparatus for firing air by electricity. This globe was well exhausted by an air-pump, and then filled with a mixture of inflammable and dephlogisticated air, by shutting the cock, fastening a bent glass tube to its mouth, and letting up the end of it into a glass jar inverted into water, and containing a mixture of 19,500 grain measures of dephlogisticated air, and 37,000 of inflammable; so that, upon opening the cock, some of this mixed air rushed through the bent tube, and filled the globe. The cock was then shut, and the included air fired by electricity, by which means almost all of it lost its elasticity. The cock was then again opened, so as to let in more of the same air, to supply the place of that destroyed by the explosion, which was again fired, and the operation continued till almost the whole of the mixture was let into the globe and exploded. By this means, though the globe held not more than the sixth part of the mixture, almost the whole of it was exploded therein, without any fresh exhaustion of the globe.

As I was desirous to try the quantity and test of this burnt air, without letting any water into the globe, which would have prevented my examining the nature of the condensed matter, I took a larger globe, furnished also with a stop cock, exhausted it by an air-pump, and screwed it on upon the cock of the former globe; upon which, by opening both cocks, the air rushed out of the smaller globe into the larger, till it became of equal density in both; then, by shutting the cock of the larger globe, unscrewing it again from the former, and opening it under water, I was enabled to find the quantity of the burnt air in it; and consequently, as the proportion which the contents of the two globes bore to each other was known, could tell the quantity of burnt air in the small globe before the communication was made between them. By this means the whole quantity of the burnt air was found to be 2,950 grain measures; its standard was 1.85.

The liquor condensed in the globe, in weight about thirty grains, was sensibly acid to the taste, and by saturation with fixed alkali, and evaporation, yielded near two grains of nitre; so that it consisted of water united to a small quantity of nitrous acid. No sooty matter was deposited in the globe. The dephlogisticated air used in this experiment was procured from red precipitate, that is, from a solution of quicksilver in spirit of nitre distilled till it acquires a red colour.

As it was suspected, that the acid contained in the condensed liquor was no essential part of the dephlogisticated air, but was owing to some acid vapour which came over in making it and had not been absorbed by the water, the experiment was repeated in the same manner, with some more of the same air, which had been previously washed with water, by keeping it a day or two in a bottle with some water, and shaking it frequently; whereas that used in the preceding experiment had never passed through water, except in preparing it. The condensed liquor was still acid.

The experiment was also repeated with dephlogisticated air, procured from red lead by means of oil of vitriol; the liquor condensed was acid, but by an accident I was prevented from determining the nature of the acid.

I also procured some dephlogisticated air from the leaves of plants, in the manner of Doctors Ingenhousz and Priestley, and exploded it with inflammable air as before; the condensed liquor still continued acid, and of the nitrous kind.

In all these experiments the proportion of inflammable air was such, that the burnt air was not much phlogisticated; and it was observed, that the less phlogisticated it was, the more acid was the condensed liquor. I therefore made another experiment, with some more of the same air from plants, in which the proportion of inflammable air was greater, so that the burnt air was almost completely phlogisticated, its standard being 1-10. The condensed liquor was then not at all acid, but seemed pure water; so that it appears, that with this kind of dephlogisticated air, the condensed liquor is not at all acid, when the two airs are mixed in such a proportion that the burnt air is almost completely phlogisticated, but is considerably so when it is not much phlogisticated.

In order to see whether the same thing would obtain with air procured from red precipitate, I made two more experiments with that

kind of air, the air in both being taken from the same bottle, and the experiment tried in the same manner, except that the proportions of inflammable air were different. In the first, in which the burnt air was almost completely phlogisticated, the condensed liquor was not at all acid. In the second, in which its standard was 1.86, that is, not much phlogisticated, it was considerably acid; so that with this air, as well as with that from plants, the condensed liquor contains, or is entirely free from, acid, according as the burnt air is less or more phlogisticated; and there can be little doubt but that the same rule obtains with any other kind of dephlogisticated air.

In order to see whether the acid, formed by the explosion of dephlogisticated air obtained by means of the vitriolic acid, would also be of the nitrous kind, I procured some air from turbith mineral, and exploded it with inflammable air, the proportion being such that the burnt air was not much phlogisticated. The condensed liquor manifested an acidity, which appeared, by saturation with a solution of salt of tartar, to be of the nitrous kind; and it was found, by the addition of some *terra ponderosa salita*, to contain little or no vitriolic acid.

When inflammable air was exploded with common air, in such a proportion that the standard of the burnt air was about 4-10, the condensed liquor was not in the least acid. There is no difference, however, in this respect between common air, and dephlogisticated air mixed with phlogisticated in such a proportion as to reduce it to the standard of common air; for some dephlogisticated air from red precipitate, being reduced to this standard by the addition of perfectly phlogisticated air, and then exploded with the same proportion of inflammable air as the common air was in the foregoing experiment, the condensed liquor was not in the least acid.

From the foregoing experiments it appears, that when a mixture of inflammable and dephlogisticated air is exploded in such proportion that the burnt air is not much phlogisticated, the condensed liquor contains a little acid, which is always of the nitrous kind, whatever substance the dephlogisticated air is procured from; but if the proportion be such that the burnt air is almost entirely phlogisticated, the condensed liquor is not at all acid, but seems pure water, without any addition whatever; and as, when they are mixed in that proportion, very little air remains after the explosion, almost the whole being condensed, it follows that almost the whole of the inflammable and

dephlogisticated air is converted into pure water. It is not easy, indeed, to determine from these experiments what proportion the burnt air, remaining after the explosions, bore to the dephlogisticated air employed, as neither the small nor the large globe could be perfectly exhausted of air, and there was no saying with exactness what quantity was left in them; but in most of them, after allowing for this uncertainty, the true quantity of burnt air seemed not more than 1-17 of the dephlogisticated air employed, or 1-50 of the mixture. It seems, however, unnecessary to determine this point exactly, as the quantity is so small, that there can be little doubt but that it proceeds only from the impurities mixed with the dephlogisticated and inflammable air, and consequently that, if those airs could be obtained perfectly pure, the whole would be condensed.

With respect to common air, and dephlogisticated air reduced by the addition of phlogisticated air to the standard of common air, the case is different; as the liquor condensed in exploding them with inflammable air, I believe I may say in any proportion, is not at all acid; perhaps because if they are mixed in such a proportion as that the burnt air is not much phlogisticated, the explosion is too weak, and not accompanied with sufficient heat.

All the foregoing experiments, on the explosion of inflammable air with common and dephlogisticated airs, except those which relate to the cause of the acid found in the water, were made in the summer of the year 1781, and were mentioned by me to Dr. Priestley, who in consequence of it made some experiments of the same kind, as he relates in a paper printed in the preceding volume of the Transactions. During the last summer also, a friend of mine gave some account of them to M. Lavoisier, as well as of the conclusion drawn from them that dephlogisticated air is only water deprived of phlogiston; but at that time so far was M. Lavoisier from thinking any such opinion warranted, that, till he was prevailed upon to repeat the experiment himself, he found some difficulty in believing that nearly the whole of the two airs could be converted into water. It is remarkable, that neither of these gentlemen found any acid in the water produced by the combustion; which might proceed from the latter having burnt two airs in a different manner from what I did; and from the former having used a different kind of inflammable air, namely, that from charcoal, and perhaps having used a greater proportion of it.

XV

SIR WILLIAM HERSCHEL

1738-1822

Sir William Herschel was born in Hanover, Germany, November 15, 1738, the son of a bandmaster. At an early age he was compelled to earn his own living by playing in the band of the Hanoverian Guards. In 1766, after some years of financial straits, he found work as an organist at Bath. Studying languages and mathematics without assistance from tutors, he became interested in "the music of the spheres" which developed into a scientific attitude in astronomy. He managed, in spite of his poverty, to construct specula for a telescope and in 1781, with one of his own instruments, he discovered the planet Uranus, one of the most romantic discoveries in the history of science. Among his other discoveries were two of the satellites of Uranus, two more of Saturn, and the fact that the moon was without atmosphere; he also described many of the binary stars, discovered many nebulous stars (which prepared the way for the nebular theory of the universe), and made the inference from the movements of the stars that the whole solar system was rushing towards the constellation of Hercules. After his death, August 25, 1822, his son, Sir John Herschel, continued his work in astronomy.

I

THE DISCOVERY OF URANUS *

ACCOUNT OF A COMET

On Tuesday, the 13th of March, 1781, between 10 and 11 in the evening, while examining the small stars in the neighborhood of H

* This excerpt and the one following are from the report by Herschel in the *Transactions of the Royal Society of London*; the third is an abstract from the same report, the conclusion, however, being by Herschel.

Geminorum, I perceived one that appeared visibly larger than the rest: being struck with its uncommon magnitude, I compared it to H Germinorum and the small star in the quartile between Auriga and Gemini, and finding it so much larger than either of them, suspected it to be a comet. I was then engaged in a series of observations on the parallax of the fixed stars, which I hope soon to have the honour of laying before the R.S., and those observations requiring very high powers, I had ready at hand several magnifiers of 227, 460, 932, 1536, 2010, &c., all of which I have successfully used on that occasion. The power I had on when I first saw the comet was 227. From experience I knew that the diameters of the fixed stars are not proportionally magnified with higher powers, as the planets are; I therefore now put on the powers of 460 and 932, and found the diameter of the comet increased in proportion to the power, as it ought to be, on the supposition of its not being a fixed star, while the diameters of the stars to which I compared it, were not increased in the same ratio. Also, that the comet being magnified much beyond what its light would admit of, appeared hazy and ill-defined with these great powers, while the stars preserved that lustre and distinctness which from many thousand observations I knew they would retain. The sequel has shown that my surmises were well founded, this proving to be the comet we have lately observed.

II

ON THE NAME OF THE NEW PLANET

By the observations of the most eminent astronomers in Europe it appears that the new star, which I had the honour of pointing out to them in March, 1781, is a primary planet of our solar system. A body so nearly related to us by its similar condition and situation, in the unbounded expanse of the starry heavens, must often be the subject of conversation, not only of astronomers, but of every lover of science in general. This consideration, then, makes it necessary to give it a name, by which it may be distinguished from the rest of the planets and fixed stars. In the fabulous ages of ancient times the appellations of Mercury, Venus, Mars, Jupiter, and Saturn, were given to the planets, as being the names of their principal heroes and

divinities. In the present more philosophical era, it would hardly be allowable to have recourse to the same method, and call on Juno, Apollo, Pallas or Minerva, for a name to our new heavenly body. The first consideration in any particular event, or remarkable incident, seems to be its chronology; if in any future age it should be asked, when this last-found planet was discovered it would be a very satisfactory answer to say, "In the reign of King George the Third." As a philosopher, then, the name of *Georgium Sidus* presents itself to me, as an appellation which will conveniently convey the information of the time and country where and when it was brought to view.

III

ON NEBULOUS STARS, PROPERLY SO CALLED

In one of his late examinations of a space in the heavens, which he had not reviewed before, Dr. H. discovered a star of about the eighth magnitude, surrounded with a faintly luminous atmosphere, of a considerable extent. The phenomenon was so striking that he could not help reflecting on the circumstance that attended it, which appeared to be of a very instructive nature, and such as might lead to inferences which will throw a considerable light on some points relating to the construction of the heavens.

Cloudy or nebulous stars have been mentioned by several astronomers; but this name ought not to be applied to the objects which they have pointed out as such; for, on examination, they proved to be either mere clusters of stars, plainly to be distinguished with his large instruments, or such nebulous appearances as might be reasonably supposed to be occasioned by a multitude of stars at a vast distance. The milky way itself consists entirely of stars, and by imperceptible degrees he was led on from most evident congeries of stars to other groups in which the lucid points were smaller, but still very plainly to be seen; and from them to such wherein they could but barely be suspected, till he arrived at last to spots in which no trace of a star was to be discerned. But then the gradations to these later were by such well-connected steps as left no room for doubt but that all these phenomena were equally occasioned by stars, variously dispersed in the immense expanse of the universe.

When Dr. H. pursued these researches, he was in the situation of a natural philosopher who follows the various species of animals and insects from the height of their perfection down to the lowest ebb of life; when, arriving at the vegetable kingdom, he can scarcely point out to us the precise boundary where the animal ceases and the plant begins; and may even go so far as to suspect them not to be essentially different. But recollecting himself, he compares, for instance, one of the human species to a tree, and all doubt of the subject vanishes before him. In the same manner we pass through gentle steps from a coarse cluster of stars, such as the Pleiades, the Præserpe, the milky way, the cluster in the Crab, the nebula in Hercules, that near the preceding hip of Bootis, the 17th, 38th, 41st of the 7th class of his catalogues, the 10th, 20th, 35th of the 6th class, the 33d, 48th, 213th of the 1st, the 12th, 150th, 756th of the 2d, and the 18th, 140th, 725th of the 3d, without any hesitation, till we find ourselves brought to an object such as the nebula in Orion, where we are still inclined to remain in the once adopted idea, of stars exceedingly remote, and inconceivably crowded, as being the occasion of that remarkable appearance. It seems, therefore, to require a more dissimilar object to set us right again. A glance like that of the naturalist, who casts his eye from the perfect animal to the perfect vegetable, is wanting to remove the veil from the mind of the astronomer. The object mentioned above is the phenomenon that was wanting for this purpose. View, for instance, the 19th cluster of the 6th class, and afterwards cast your eye on this cloudy star, and the result will be no less decisive than that of the naturalist alluded to. Our judgment will be, that the nebulosity about the star is not of a starry nature.

But that we may not be too precipitate in these new decisions, let us enter more at large into the various grounds which induced us formerly to surmise, that every visible object, in the extended and distant heavens, was of the starry kind, and collate them with those which now offer themselves for the contrary opinion. It has been observed, on a former occasion, that all the smaller parts of other great systems, such as the planets, their rings and satellites, the comets, and such other bodies of the like nature as may belong to them, can never be perceived by us, on account of the faintness of light reflected from small opaque objects: in the present remarks, therefore, all these are to be entirely set aside.

A well connected series of objects, such as mentioned above, has led us to infer that all nebulae consist of stars. This being admitted, we were authorized to extend our analogical way of reasoning a little further. Many of the nebulae had no other appearance than that whitish cloudiness, on the blue ground on which they seemed to be projected; and why the same cause should not be assigned to explain the most extensive nebulosities, as well as those that amounted only to a few minutes of a degree in size, did not appear. It could not be inconsistent to call up a telescopic milky way, at an immense distance, to account for such a phenomenon; and if any part of the nebulosity seemed detached from the rest, or contained a visible star or two, the probability of seeing a few near stars, apparently scattered over the far distant regions of myriads of sidereal collections, rendered nebulous by their distance, would also clear up these singularities.

In order to be more easily understood in his remarks on the comparative disposition of the heavenly bodies, Dr. H. mentions some of the particulars which introduced the ideas of connection and disjunction: for these, being properly founded on an examination of objects that may be reviewed at any time, will be of considerable importance to the validity of what we may advance with regard to the lately discovered nebulous stars. On June 27, 1786, he saw a beautiful cluster of very small stars of various sizes, about 15' in diameter, and very rich of stars. On viewing this object, it is impossible to withhold our assent to the idea which occurs, that these stars are connected so far with one another as to be gathered together, within a certain space, of little extent when compared to the vast expanse of the heavens. As this phenomenon has been repeatedly seen in a thousand cases, Dr. H. thinks he may justly lay great stress on the idea of such stars being connected. On September 9, 1779, he discovered a very small star near ϵ Bootis. The question here occurring, whether it had any connection with ϵ or not, was determined in the negative; for, considering the number of stars scattered in a variety of places, it is very far from being uncommon, that a star at a great distance should happen to be nearly in a line drawn from the sun through ϵ , and thus constitute the observed double star. September 7, 1782, when Dr. H. first saw the planetary nebula near ν Aquarii, he pronounced it to be a system whose parts

were connected together. Without entering into any kind of calculation, it is evident that a certain degree of light within a very small space, joined to the particular shape this object presents to us, which is nearly round, and even in its deviation consistent with regularity, being a little elliptical, ought naturally to give us the idea of a conjunction in the things that produce it. And a considerable addition to this argument may be derived from a repetition of the same phenomenon, in nine or ten more of a similar construction.

When Dr. H. examined the cluster of stars, following the head of the Great Dog, he found on March 19, 1786, that there was within this cluster a round, resolvable nebula, of about 2' in diameter, and nearly an equal degree of light throughout. Here, considering that the cluster was free from nebulosity in other parts, and that many such clusters, as well as such nebulae, exist in divers parts of the heavens, it seemed very probable that the nebula was unconnected with the cluster; and that a similar reason would as easily account for this appearance as it had resolved the phenomenon of the double star near *e* Bootis; that is, a casual situation of our sun and the two other objects nearly in a line. And though it may be rather more remarkable, that this should happen with two compound systems, which are not by far so numerous as single stars, we have, to make up for this singularity, a much larger space in which it may take place, the cluster being of a very considerable extent.

On February 15, 1786, Dr. H. discovered that one of his planetary nebulae had a spot in the centre, which was more luminous than the rest, and with long attention, a very bright, round, well-defined centre became visible. He remained not a single moment in doubt, but that the bright centre was connected with the rest of the apparent disc. October 6, 1785, he found a very bright, round nebula, of about $1\frac{1}{2}'$ in diameter. It has a large, bright nucleus in the middle, which is undoubtedly connected with the luminous parts about it. And though we must confess, that if this phenomenon, and many more of the same nature, recorded in the catalogues of nebulae, consist of clustering stars, we find ourselves involved in some difficulty to account for the extraordinary condensation of them about the centre; yet the idea of a connection between the outward parts and these very condensed ones within, is by no means lessened on that account.

There is a telescopic milky way, which Dr. H. has traced out in the heavens in many sweeps made from the year 1783 to 1789. It takes up a space of more than 60 square degrees of the heavens, and there are thousands of stars scattered over it: among others, four that form a trapezium, and are situated in the well known nebula of Orion, which is included in the above extent. All these stars, as well as the four mentioned, he takes to be entirely unconnected with the nebulosity which involves them in appearance. Among them is also *d* Orionis, a cloudy star, improperly so called by former astronomers; but it does not seem to be connected with the milkiess any more than the rest.

Dr. H. now comes to some other phenomena, that, from their singularity, merit undoubtedly a very full discussion. Among the reasons which induced us to embrace the opinion that all very faint milky nebulosity ought to be ascribed to an assemblage of stars is, that we could not easily assign any other cause of sufficient importance for such luminous appearances, to reach us at the immense distance we must suppose ourselves to be from them. But if an argument of considerable force should now be brought forward, to show the existence of luminous matter, in a state of modification very different from the construction of a sun or star, all objections, drawn from our incapacity of accounting for new phenomena on old principles, he thinks, will lose their validity.

Hitherto Dr. H. has been showing, by various instances in objects whose places are given, in what manner we may form ideas of connection, and its contrary, by an attentive inspection of them only; he now relates a series of observations, with remarks on them as they are delivered, from which he afterwards draws a few simple conclusions, that seem to be of considerable importance.

October 16, 1784. A star of about the ninth magnitude, surrounded by a milky nebulosity, or chevelure, of about 3' in diameter. The nebulosity is very faint, and a little extended or elliptical, the extent being not far from the meridian, or a little from north preceding to south following. The chevelure involves a small star, which is about $1\frac{1}{2}'$ north of the cloudy star; other stars of equal magnitude are perfectly free from this appearance. (R.A. 5h 57m 4s. P.D. $96^{\circ} 22'$). His present judgment concerning this remarkable object is, that the nebulosity belongs to the star which is situated

in its centre. The small one, on the contrary, which is mentioned as involved, being one of many that are profusely scattered over this rich neighbourhood, he supposes to be quite unconnected with this phenomenon. A circle of 3' in diameter is sufficiently large to admit another small star, without any bias to the judgment he formed concerning the one in question. It might appear singular, that such an object should not have immediately suggested all the remarks contained in this paper; but about things that appear new we ought not to form opinions too hastily, and his observations on the construction of the heavens were then but entered on. In this case, therefore, it was the safest way to lay down a rule not to reason on the phenomena that might offer themselves, till he should be in possession of a sufficient stock of materials to guide his researches.

October 16, 1784. A small star of about the 11th or 12th magnitude, very faintly affected with milky nebulosity; other stars of the same magnitude were perfectly free from this appearance. Another observation mentions five or six small stars within the space of 3 or 4', all very faintly affected in the same manner, and the nebulosity suspected to be a little stronger about each star. But a third observation rather opposes this increase of the faintly luminous appearance. (R. A. 6h 0m 33s. P. D. $96^{\circ} 13'$). Here the connection between the stars and the nebulosity is not so evident as to amount to conviction; for which reason we shall pass on to the next.

November 25, 1788. A star of about the 9th magnitude, surrounded with very faint milky nebulosity; other stars of the same size are perfectly free from that appearance. Less than 1' in diameter. The star is either not round or double (a).

March 23, 1789. A bright, considerably well-defined nucleus, with a very faint, small, round chevelure (b). The connection admits of no doubt; but the object is not perhaps of the same nature with those called cloudy stars.

April 14, 1789. A considerable, bright, round nebula; having a large place in the middle of nearly an equal brightness; but less bright towards the margin (c). This seems rather to approach the planetary sort.

March 5, 1790. A pretty considerable star of the 9th or 10th

magnitude, visibly affected with a very faint nebulosity of little extent, all around. A power of 300 showed the nebulosity of greater extent (d). The connection is not to be doubted.

March 19, 1790. A very bright nucleus, with a small, very faint chevelure, exactly round. In a low situation, where the chevelure could hardly be seen, this object would put on the appearance of an ill-defined, planetary nebula, of 6, 8 or 10" diameter (e).

November 13, 1790. A most singular phenomenon! A star of about the 8th magnitude, with a faint luminous atmosphere, of a circular form, and of about 3' in diameter. The star is perfectly in the centre, and the atmosphere is so diluted, faint, and equal throughout, that there can be no surmise of its consisting of stars; nor can there be a doubt of the evident connection between the atmosphere and the star. Another star not much less in brightness, and in the same field with the above, was perfectly free from any such appearance. This last object is so decisive in every particular, Dr. H. says, that we need not hesitate to admit it as a pattern, from which we are authorised to draw the following important consequences:

Supposing the connection between the star and its surrounding nebulosity to be allowed, we argue, that one of the two following cases must necessarily be admitted: In the first place, if the nebulosity consist of stars that are very remote, which appear nebulous on account of the small angles their mutual distances subtend at the eye, by which they will not only, as it were, run into each other, but also appear extremely faint and diluted; then, what must be the enormous size of the central point, which outshines all the rest in so superlative a degree as to admit of no comparison! In the next place, if the star be larger than common, how very small and compressed must be those other luminous points that are the occasion of the nebulosity which surrounds the central one! As, by the former supposition, the luminous central point must far exceed the standard of what we call a star, so, in the latter, the shining matter about the centre will be much too small to come under the same denomination; we therefore either have a central body which is not a star, or have a star which is involved in a shining fluid, of a nature totally unknown to us. Dr. H. can adopt no other sentiment than the latter, since the probability is certainly not for the existence of so enormous a body as would

be required to shine like a star of the eighth magnitude, at a distance sufficiently great to cause a vast system of stars to put on the appearance of a very diluted milky nebulosity.

But what a field of novelty is here opened to our conceptions! A shining fluid, of a brightness sufficient to reach us from the remote regions of a star of the 8th, 9th, 10th, or 12th magnitude, and of an extent so considerable as to take up 3, 4, 5, or 6 minutes in diameter! Can we compare it to the coruscation of the electric fluid in the aurora borealis? Or to the more magnificent cone of the zodiacal light as we see it in the spring or autumn? The latter, notwithstanding Dr. H. has observed it to reach at least 90° from the sun, is yet of so little extent and brightness, as probably not to be perceived even by the inhabitants of Saturn or the Georgian planet, and must be utterly invisible at the remoteness of the nearest fixed star.

More extensive views may be derived from this proof of the existence of a shining matter. Perhaps it has been too hastily surmised that all milky nebulosity, of which there is so much in the heavens, is owing to starlight only. These nebulous stars may serve as a clue to unravel other mysterious phenomena. If the shining fluid that surrounds them is not so essentially connected with these nebulous stars, but that it can also exist without them, which seems to be sufficiently probable, and will be examined hereafter, we may with great facility explain that very extensive, telescopic nebulosity, which, as before mentioned, is expanded over more than 60° of the heavens, about the constellation of Orion; a luminous matter accounting much better for it than clustering stars at a distance. In this case we may also pretty nearly guess at its situation, which must commence somewhere about the range of the stars of the 7th magnitude, or a little farther from us, and extend unequally in some places perhaps to the regions of those of the 9th, 10th, 11th, and 12th. The foundation for this surmise is, that not unlikely some of the stars that happen to be situated in a more condensed part of it, or that perhaps by their own attraction draw together some quantity of this fluid greater than what they are entitled to by their situation in it, will, of course, assume the appearance of cloudy stars; and many of those named are either in this stratum of luminous matter, or very near it.

It has been said above, that in nebulous stars the existence of the shining fluid does not seem to be so essentially connected with the

central points that it might not also exist without them. For this opinion we may assign several reasons. One of them is the greater resemblance of the chevelure of these stars and the diffused extensive nebulosity mentioned before, which renders it highly probable that they are of the same nature. Now, if this be admitted, the separate existence of the luminous matter, or its independence of a central star, is fully proved. We may also judge, very confidently, that the light of this shining fluid is no kind of reflection from the star in the centre; for, as we have already observed, reflected light could never reach us at the great distance we are from such objects. Besides, how impenetrable would be an atmosphere of a sufficient density to reflect so great a quantity of light! And yet we observe, that the outward parts of the chevelure are nearly as bright as those that are close to the star; so that this supposed atmosphere ought to give no obstruction to the passage of the central rays. If therefore this matter is self-luminous, it seems more fit to produce a star by its condensation than to depend on the star for its existence.

Many other diffused nebulosities, besides that about the constellation of Orion, have been observed or suspected; but some of them are probably very distant, and run far out into space. For instance, about 5m in time preceding α Cygni, Dr. H. suspects as much of it as covers near 4 square degrees; and much about the same quantity 44m preceding the 125 Tauri. A space of almost 8 square degrees, 6m preceding α Trianguli, seems to be tinged with milky nebulosity. Three minutes preceding the 46 Eridani, strong, milky nebulosity is expanded over more than 2 square degrees. Fifty-four minutes preceding the 13th *Canum venaticorum*, and again 48m preceding the same star, the field of view affected with whitish nebulosity throughout the whole breadth of the sweep, which was $2^{\circ} 39'$. Four minutes following the 57 Cygni a considerable space is filled with faint, milky nebulosity, which is pretty bright in some places, and contains the 37th nebula of the 5th class, in the brightest part of it. In the neighbourhood of the 44th Piscium, very faint nebulosity appears to be diffused over more than 9 square degrees of the heavens. Now all these phenomena, as we have already seen, will admit of a much easier explanation by a luminous fluid than by stars at an immense distance.

The nature of planetary nebulæ, which has hitherto been involved

in much darkness, may now be explained with some degree of satisfaction, since the uniform and very considerable brightness of their apparent disc accords remarkably well with a much condensed, luminous fluid; whereas, to suppose them to consist of clustering stars, will not so completely account for the milky or soft tint of their light, to produce which it would be required that the condensation of the stars should be carried to an almost inconceivable degree of accumulation. The surmise of the regeneration of stars, by means of planetary nebulae, expressed in a former paper, will become more probable, as all the luminous matter contained in one of them, when gathered together into a body of the size of a star, would have nearly such a quantity of light as we find the planetary nebulae to give. To prove this experimentally, we may view them with a telescope that does not magnify sufficiently to show their extent, by which means we shall gather all their light together into a point, when they will be found to assume the appearance of small stars; that is, of stars at the distance of those which we call of the 8th, 9th, or 10th magnitude. Indeed this idea is greatly supported by the discovery of a well-defined, lucid point, resembling a star, in the centre of one of them; for the argument which has been used, in the case of nebulous stars, to show the probability of the existence of luminous matter, which rested on the disparity between a bright point and its surrounding shining fluid, may here be alleged with equal justice. If the point be a generating star, the further accumulation of the already much condensed, luminous matter may complete it in time.

How far the light that is perpetually emitted from millions of suns may be concerned in this shining fluid, it might be presumptuous to attempt to determine; but, notwithstanding the inconceivable subtilty of the particles of light, when the number of the emitting bodies is almost infinitely great, and the time of the continual emission indefinitely long, the quantity of emitted particles may well become adequate to the constitution of a shining fluid, or luminous matter, provided a cause can be found that may retain them from flying off, or reunite them. But such a cause cannot be difficult to guess at, when we know that light is so easily reflected, refracted, inflected and deflected; and that, in the immense range of its course, it must pass through innumerable systems, where it cannot but frequently meet with many obstacles to its rectilinear progression not to mention

the great counteraction of the united attractive force of whole sidereal systems, which must be continually exerting their power on the particles while they are endeavouring to fly off. However, we shall lay no stress on a surmise of this kind, as the means of verifying it are wanting; nor is it of any immediate consequence to us to know the origin of the luminous matter. Let it suffice, that its existence is rendered evident, by means of nebulous stars.

XVI

KARL WILHELM SCHEELÉ

1742-1786

Karl Wilhelm Scheele, who discovered independently of the English chemists the double constitution of air, was born in Stralsund, Pomerania, December 19, 1742. At an early age he manifested interest in pharmacy, and during his career as an apothecary engaged in various experiments in chemistry. He published his "Treatise on Air and Fire" in 1777. He died at Köping, May 21, 1786.

THE CONSTITUENTS OF AIR *

1. It is the object and chief business of chemistry to separate skilfully substances into their constituents, to discover their properties, and to compound them in different ways. How difficult it is, however, to carry out such operations with the greatest accuracy, can only be unknown to one who either has never undertaken this occupation, or at least has not done so with sufficient attention.

2. Hitherto chemical investigators are not agreed as to how many elements or fundamental materials compose all substances. In fact this is one of the most difficult problems; some indeed hold that there remains no further hope of searching out the elements of substances. Poor comfort for those who feel their greatest pleasure in the investigation of natural things! Far is he mistaken, who endeavours to confine chemistry, this noble science, within such narrow bounds! Others believe that earth and phlogiston are the things from which all material nature has derived its origin. The majority seem completely attached to the peripatetic elements.

3. I must admit that I have bestowed no little trouble upon this

* Translated from *Treatise on Air and Fire* (1777).

matter in order to obtain a clear conception of it. One may reasonably be amazed at the ideas and conjectures which authors have recorded on the subject, especially when they give a decision respecting the phenomenon of fire; and this very matter was of the greatest importance to me. I perceived the necessity of a knowledge of fire, because without this it is not possible to make any experiment; and without fire and heat it is not possible to make use of the action of any solvent. I began accordingly to put aside all explanations of fire; I undertook a multitude of experiments in order to fathom this beautiful phenomenon as fully as possible. I soon found, however, that one could not form any true judgment regarding the phenomena which fire presents, without a knowledge of the air. I saw, after carrying out a series of experiments, that air really enters into the mixture of fire, and with it forms a constituent of flame and of sparks. I learned accordingly that a treatise like this, on fire, could not be drawn up with proper completeness without taking the air also into consideration.

4. Air is that fluid invisible substance which we continually breathe, which surrounds the whole surface of the earth, is very elastic, and possesses weight. It is always filled with an astonishing quantity of all kinds of exhalations, which are so finely subdivided in it that they are scarcely visible even in the sun's rays. Water vapours always have the preponderance amongst these foreign particles. The air, however, is also mixed with another elastic substance resembling air, which differs from it in numerous properties, and is, with good reason, called aerial acid by Professor Bergman. It owes its presence to organised bodies, destroyed by putrefaction or combustion.

5. Nothing has given philosophers more trouble for some years than just this delicate acid or so-called fixed air. Indeed it is not surprising that the conclusions which one draws from the properties of this elastic acid are not favourable to all who are prejudiced by previously conceived opinions. These defenders of the Paracelsian doctrine believe that the air is in itself unalterable; and, with Hales, that it really unites with substances thereby losing its elasticity; but that it regains its original nature as soon as it is driven out of these by fire or fermentation. But since they see that the air so produced is endowed with properties quite different from common air, they conclude, without experimental proofs, that this air has united with

foreign materials, and that it must be purified from these admixed foreign particles by agitation and filtration with various liquids. I believe that there would be no hesitation in accepting this opinion, if one could only demonstrate clearly by experiments that a given quantity of air is capable of being completely converted into fixed or other kind of air by the admixture of foreign materials; but since this has not been done, I hope I do not err if I assume as many kinds of air as experiment reveals to me. For when I have collected an elastic fluid, and observe concerning it that its expansive power is increased by heat and diminished by cold, while it still uniformly retains its elastic fluidity, but also discover in it properties and behavior different from those of common air, then I consider myself justified in believing that this is a peculiar kind of air. I say that air thus collected must retain its elasticity even in the greatest cold, because otherwise an innumerable multitude of varieties of air would have to be assumed, since it is very probable that all substances can be converted by excessive heat into a vapour resembling air.

6. Substances which are subjected to putrefaction or to destruction by means of fire diminish, and at the same time consume, a part of the air; sometimes it happens that they perceptibly increase the bulk of the air, and sometimes finally that they neither increase nor diminish a given quantity of air—phenomena which are certainly remarkable. Conjectures can here determine nothing with certainty, at least they can only bring small satisfaction to a chemical philosopher, who must have his proofs in his hands. Who does not see the necessity of making experiments in this case, in order to obtain light concerning this secret of nature?

7. General properties of ordinary air.

(1.) Fire must burn for a certain time in a given quantity of air. (2.) If, so far as can be seen, this fire does not produce during combustion any fluid resembling air, then, after the fire has gone out of itself, the quantity of air must be diminished between a third and a fourth part. (3.) It must not unite with common water. (4.) All kinds of animals must live for a certain time in a confined quantity of air. (5.) Seeds, as for example peas, in a given quantity of similarly confined air, must strike roots and attain a certain height with the aid of some water and of a moderate heat.

Consequently, when I have a fluid resembling air in its external

appearance, and find that it has not the properties mentioned, even when only one of them is wanting, I feel convinced that it is not ordinary air.

8. Air must be composed of elastic fluids of two kinds.

First Experiment.—I dissolved one ounce of alkaline liver of sulphur in eight ounces of water; I poured four ounces of this solution into an empty bottle capable of holding 24 ounces of water, and closed it most securely with a cork; I then inverted the bottle and placed the neck in a small vessel with water; in this position I allowed it to stand for fourteen days. During this time the solution had lost a part of its red colour and had also deposited some sulphur: afterwards I took the bottle and held it in the same position in a larger vessel with water, so that the mouth was under and the bottom above the water-level, and withdrew the cork under the water; immediately water rose with violence into the bottle. I closed the bottle again, removed it from the water, and weighed the fluid which it contained. There were 10 ounces. After subtracting from this the four ounces of solution of sulphur there remain six ounces, consequently it is apparent from this experiment that of 20 parts of air six parts have been lost in 14 days.

9. Second Experiment.—(a) I repeated the preceding experiment with the same quantity of liver of sulphur, but with this difference that I only allowed the bottle to stand a week tightly closed. I then found that of 20 parts of air only 4 had been lost. (b) On another occasion I allowed the very same bottle to stand four months; the solution still possessed a somewhat dark yellow colour. But no more air had been lost than in the first experiment, that is to say six parts.

10. Third Experiment.—I mixed two ounces of caustic ley, which was prepared from alkali of tartar and unslaked lime and did not precipitate lime-water, with half an ounce of the preceding solution of sulphur, which likewise did not precipitate lime-water. This mixture had a yellow colour. I poured it into the same bottle, and after this had stood fourteen days, well closed, I found the mixture entirely without colour and also without precipitate. I was enabled to conclude that the air in this bottle had likewise diminished, from the fact that air rushed into the bottle with a hissing sound after I had made a small hole in the cork.

11. Fourth Experiment.—(a) I took four ounces of a solution of

sulphur in lime-water; I poured this solution into a bottle and closed it tightly. After 14 days the yellow colour had disappeared, and of 20 parts of air 4 parts had been lost. The solution contained no sulphur, but had allowed a precipitate to fall which was chiefly gypsum. (b.) Volatile liver of sulphur likewise diminishes the bulk of air. (c) Sulphur, however, and volatile spirit of sulphur, undergo no alteration in it.

12. Fifth Experiment.—I hung up over burning sulphur, linen rags which were dipped in a solution of alkali of tartar. After the alkali was saturated with the volatile acid, I placed the rags in a flask, and closed the mouth most carefully with a wet bladder. After three weeks had elapsed I found the bladder strongly pressed down; I inverted the flask, held its mouth in water and made a hole in the bladder; thereupon water rose with violence into the flask and filled the fourth part.

13. Sixth Experiment.—I collected in the bladder the nitrous acid which arises on the dissolution of the metals in nitrous acid, and after I had tied the bladder tightly I laid it in a flask and secured the mouth very carefully with a wet bladder. The nitrous air gradually lost its elasticity, the bladder collapsed, and became yellow as if corroded by *aqua fortis*. After 14 days I made a hole in the bladder tied over the flask, having previously held it, inverted, under water; the water rose rapidly into the flask, and it remained only two-thirds empty.

14. Seventh Experiment.—(a.) I immersed the mouth of a flask in a vessel with oil of turpentine. The oil rose in the flask a few lines every day. After the lapse of 14 days the fourth part of the flask was filled with it. I allowed it to stand for three weeks longer, but the oil did not rise higher. All those oils which dry in the air, and become converted into resinous substances, possess this property. Oil of turpentine, however, and linseed oil rise up sooner if the flask is previously rinsed out with a concentrated sharp ley. (b.) I poured two ounces of colourless and transparent animal oil of Dippel into a bottle and closed it very tightly; after the expiration of two months the oil was thick and black. I then held the bottle, inverted, under water and drew out the cork; the bottle immediately became one-fourth filled with water.

15. Eighth Experiment.—(a.) I dissolved two ounces of vitriol of iron in thirty-two ounces of water, and precipitated this solution with a

caustic ley. After the precipitate had settled, I poured away the clear fluid and put the dark green precipitate of iron so obtained, together with the remaining water, into the before-mentioned bottle (§ 8), and closed it tightly. After 14 days (during which time I shook the bottle frequently) this green calx of iron had acquired the colour of crocus of iron, and of 40 parts of air 12 had been lost. (b.) When iron filings are moistened with some water and preserved for a few weeks in a well closed bottle, a portion of the air is likewise lost. (c.) The solution of iron in vinegar has the same effect upon air. In this case the vinegar permits the dissolved iron to fall out in the form of a yellow crocus, and becomes completely deprived of this metal. (d.) The solution of copper prepared in closed vessels with spirit of salt likewise diminishes air. In none of the foregoing kinds of air can either a candle burn or the smallest spark glow.

16. It is seen from these experiments that phlogiston, the simple inflammable principle, is present in each of them. It is known that the air strongly attracts to itself the inflammable part of substances and deprives them of it: not only this may be seen from the experiments cited, but it is at the same time evident that on the transference of the inflammable substance to the air a considerable part of the air is lost. But that inflammable substance alone is the cause of this action, is plain from this, that, according to the tenth paragraph, not the least trace of sulphur remains over, since, according to my experiments this colourless ley contains only some vitriolated tartar. The eleventh paragraph likewise shows this. But since sulphur alone, and also the volatile spirit of sulphur, have no effect upon the air (§ 11. c), it is clear that the decomposition of liver of sulphur takes place according to the laws of double affinity—that is to say, that the alkalies and lime attract the vitriolic acid, and the air attracts the phlogiston.

It may also be seen from the above experiments, that a given quantity of air can only unite with, and at the same time saturate, a certain quantity of the inflammable substance: this is evident from the ninth paragraph, letter b. But whether the phlogiston which was lost by the substances was still present in the air left behind in the bottle, or whether the air which was lost had united and fixed itself with the materials such as liver of sulphur, oils, &c., are questions of importance.

From the first view, it would necessarily follow that the inflam-

mable substance possessed the property of depriving the air of part of its elasticity, and that in consequence of this it becomes more closely compressed by the external air. In order now to help myself out of these uncertainties, I formed the opinion that any such air must be specifically heavier than ordinary air, both on account of its containing phlogiston and also of its greater condensation. But how perplexed was I when I saw that a very thin flask which was filled with this air, and most accurately weighed, not only did not counterpoise an equal quantity of ordinary air, but was even somewhat lighter. I then thought that the latter view might be admissible; but in that case it would necessarily follow also that the lost air could be separated again from the materials employed. None of the experiments cited seemed to me capable of showing this more clearly than that according to the tenth paragraph, because this residuum, as already mentioned, consists of vitriolated tartar and alkali. In order therefore to see whether the lost air had been converted into fixed air, I tried whether the latter shewed itself when some of the caustic ley was poured into lime-water; but in vain—no precipitation took place. Indeed, I tried in several ways to obtain the lost air from this alkaline mixture, but as the results were similar to the foregoing, in order to avoid prolixity I shall not cite these experiments. Thus much I see from the experiments mentioned, that the air consists of two fluids, differing from each other, the one of which does not manifest in the least the property of attracting phlogiston, while the other, which composes between the third and the fourth part of the whole mass of the air, is peculiarly disposed to such attraction. But where this latter kind of air has gone to after it has united with the inflammable substance, is a question which must be decided by further experiments, and not by conjectures.

XVII

ANTOINE LAURENT LAVOISIER

1743-1794

Antoine Laurent Lavoisier was born in Paris, August 26, 1743. After an early life spent in diligent study, in 1766 he was awarded a prize for his essay on the best method of lighting Paris. His attention having been called to the English experiments on gases made by Priestley and Cavendish, he attacked the current phlogiston conception of combustion and stated that Priestley's "dephlogisticated air" was the universal acidifying gas, and gave it the name of "oxygen." Systematizing chemistry and renaming the elements and their compounds, he came to believe that chemical reactions had the certainty of mathematical equations. From this he derived the idea of the persistence of matter, regardless of changes, now established as one of the basic concepts of modern science. During the French Revolution a charge was brought against him and he was sent to the guillotine on May 8, 1794.

THE NATURE OF COMBUSTION *

I venture to submit to the Academy today a new theory of combustion, or rather, to speak with that reserve to whose law I submit myself, an hypothesis, by the aid of which all the phenomena of combustion, calcination, and even to some extent those accompanying the respiration of animals are explained in a very satisfactory manner. I had already laid the foundations of this hypothesis p. 279-280 of vol. I. of my *Opuscules physiques et chimiques*; but I admit that trusting little to my own knowledge, I did not then dare to put forward an opinion which might seem singular, and which was directly

* *On Combustion*, Vol. II, p. 225.

opposed to the theory of Stahl and of many celebrated men who have followed him.

Though perhaps some of the reasons which then checked me still remain today, nevertheless, the facts which have multiplied since that time, and which seem to me favorable to my views, have confirmed me in my opinion: though not, perhaps, any stronger, I have become more confident, and I think I have sufficient proofs, or at least probabilities, so that even those who may not be of my opinion cannot blame me for having written.

In general in the combustion of bodies four constant phenomena are observable, which seem to be laws from which nature never departs. Though these phenomena may be found implicitly stated in other memoirs, yet I cannot dispense with recalling them here in a few words.

FIRST PHENOMENON

All combustion sets free matter either of fire or light.

SECOND PHENOMENON

Bodies can burn only in a very small number of kinds of gases (airs), or rather there can be combustion only in one kind of air, that which Mr. Priestley has named dephlogisticated air, and which I should call pure air. Not only will the bodies which we call combustibles not burn in a vacuum or in any other kind of air, they are, on the contrary, extinguished there as promptly as if they had been plunged into water or any other liquid.

THIRD PHENOMENON

In all combustion there is destruction or decomposition of the pure air in which the combustion takes place, and the body burned increases in weight exactly in proportion to the quantity of air destroyed or decomposed.

FOURTH PHENOMENON

In all combustion the body burned changes to an acid by the addition of the substance which has increased its weight: thus, for ex-

ample, if sulphur is burned under a receiver the product of the combustion is vitriolic acid; if phosphorus be burned the product is phosphoric acid; if a carboniferous substance, the product is fixed air, otherwise known as acid of lime (carbonic acid, etc.).

(Note: I would remark in passing that the number of acids is infinitely greater than has been supposed.)

The calcination of metals is subject to exactly the same laws, and it is with very great reason that Mr. Macquer has treated it as a slow combustion; thus, 1°, in all metallic combustion there is a liberating of fire matter (*matière du feu*); 2°, veritable calcination can take place only in pure air; 3°, there is a combination of the air with the substance calcined, but with this difference, that in place of forming an acid with it there results from it a particular combination known as metallic calx.

This is not the place to point out the analogy which exists between the respiration of animals, combustion and calcination; I shall return to that in the sequel to this memoir.

These different phenomena of the calcination of metals and of combustion are explained in a very happy manner by Stahl's hypothesis; but it is necessary with him to suppose the existence of fire matter (*matière du feu*) or of fixed phlogiston in the metals, in sulphur and in all bodies which he regards as combustibles; yet if the partisans of Stahl's doctrine are asked to prove the existence of fire matter in combustible bodies, they fall necessarily into a vicious circle and are obliged to reply that combustible bodies contain fire matter because they burn, and that they burn because they contain fire matter. It is easy to see that in the last analysis this is explaining combustion by combustion.

The existence of fire matter, or phlogiston, in metals, in sulphur, etc., is then really only an hypothesis, a supposition which, once admitted, explains, it is true, some of the phenomena of calcination and combustion; but if I show that these very phenomena may be explained in quite as natural a way by the opposite hypothesis, that is to say, without supposing the existence of either fire matter or phlogiston in the substances called combustible, Stahl's system will be shaken to its foundations.

No doubt you will not fail to ask me first what I understand by fire matter. I reply with Franklin, Boerhaave and some of the

philosophers of old, that the matter of fire or of light is a very subtle, very elastic fluid, which surrounds every part of the planet we live on, which penetrates with more or less ease the substances which compose that, and which tends, when it is free, to come to a state of equilibrium in all.

I will add, borrowing the chemical phraseology, that this fluid is the solvent of a large number of substances; that it combines with them in the same way that water does with salt, and the acids with metals, and that the bodies thus combined and dissolved by the igneous fluid lose in part the properties which they had before the combination and acquire new ones which bring them nearer (make them more like) the fire matter.

It is thus, as I have shown in a memoir deposited with the secretary of this Academy, that every aeriform fluid, every kind of air, is a resultant of the combination of some substance, solid or fluid, with the matter of fire or of light; and it is to this combination that aeriform fluids owe their elasticity, their specific volatility, their rarity, and all the other properties which ally (*rapprochent*) them to the igneous fluid.

Pure air, according to this, what Mr. Priestley calls dephlogisticated air, is an igneous compound into which the matter of fire or of light enters as solvent, and into which some other substance enters as a base; but if, in any solution whatever, a substance is presented to the base with which that has greater affinity, it unites with this instantly and the solvent which it leaves is set free.

The same thing happens with the air in combustion; the substance which burns steals away the base; then the fire matter which served as its solvent becomes free, regains its rights and escapes with the characteristics by which we know it; that is to say, with flame, heat and light.

To elucidate whatever may seem obscure in this theory let us apply it to some examples: when a metal is calcined in pure air, the base of the air, which has less affinity for its own solvent than for the metal, unites with the latter as it melts and converts it into metallic calx. This combination of the base of the air with the metal is proved 1st, by the increase in weight which the latter undergoes in calcination; 2nd, by the almost total using up of the air under the receiving bell.

But, if the base of the air was held in solution by the fire matter, in proportion as this base combined with the metal, the fire matter should become free and produce, in freeing itself, flame and light. You understand that the more speedy the calcination of the metal, that is to say, the more fixation of the air takes place in a given time, the more fire matter will be liberated, and, consequently, the more marked and obvious the combustion will be.

I might apply this theory successively to all combustions, but as I shall have frequent occasion to return to this subject, I will content myself at this time with these general illustrations. So, to resume, the air is composed, according to my idea, of fire matter as a solvent combined with a substance which serves it as a base, and which in some way neutralizes it; whenever a substance for which it has a greater affinity is brought into contact with this base, it leaves its solvent; then the fire-substance regains its rights, its properties, and appears to our eyes with heat, flame and light.

Pure air, the dephlogisticated air of Mr. Priestley, is then, according to this opinion, the real combustible body, and perhaps the only one of that nature, and it is seen that it is no longer necessary, in order to explain the phenomena of combustion, to suppose that there exists a large quantity of fire fixed in all the substances which we call combustible, but that it is very probable, on the contrary, that very little of it exists in metals, in sulphur, phosphorus, and in most of the very solid, heavy and compact bodies, and, perhaps even that there exists in these substances only free fire matter, in virtue of the property which this matter has of putting itself in equilibrium with all surrounding bodies.

Another striking reflection which comes to the support of the preceding ones, is that almost all substances may exist in three different states: under a solid form, under a liquid form, that is to say melted, or in the state of air or vapor. These three states depend solely on the quantity, more or less, of fire matter with which these substances are interpenetrated and with which they are combined. Fluidity, vaporization, elasticity, are then properties characteristic of the presence of fire and of a great abundance of fire; solidity, compactness, on the contrary, are evidences of its absence. By so much then

as it is demonstrated that aeriform substances and air itself contain a large quantity of fire in combination, by so much it is probable that solid bodies contain little of it.

For the rest, I repeat, in attacking here the doctrine of Stahl, it was not my purpose to substitute for it a rigorously demonstrated theory, but only an hypothesis which seemed to me more probable, more in conformity with the laws of nature, and one which appeared to involve less forced explanations and fewer contradictions.

. XVIII

ALESSANDRO VOLTA

1745-1827

Alessandro Volta, born at Como, Italy, February 18, 1745, became teacher of physics at Como in 1774, and five years later accepted a professorship at Pavia. Becoming interested in Galvani's experiments with electricity on the muscles of a frog, he applied them in his attempts to confirm his own theory that the frog's muscles were a sensitive electrometer. In doing this he conceived the voltaic pile, which produced the first constant electrical current—a discovery which had immense effects in later studies in electricity. He died at Como, March 5, 1827.

NEW GALVANIC INSTRUMENT *

ON THE ELECTRICITY EXCITED BY THE MERE CONTACT OF CONDUCTING
SUBSTANCES OF DIFFERENT KINDS

The chief of these results, and which comprehends nearly all the others, is the construction of an apparatus which resembles in its effects, viz. (such as giving shocks to the arms, &c.,) the Leyden phial, and still better, electric batteries weakly charged; acting continually, or whose charge, after each explosion, recharges itself again; which in short becomes perpetual, from one infallible charge, from one action or impulse on the electric fluid; but which besides differs essentially from the other, by this continual action which is proper to it, and because that instead of consisting, like the ordinary phials and electric batteries, in one or more isolated plates, or thin layers of those bodies deemed the only electrics, and armed with conductors or bodies called non-electrics, this new apparatus is formed only of a number of these last bodies, chosen even among the best conductors, and so the farthest removed, according to the usual opinion, from the electric principle. This astonishing apparatus is

* From the *Transactions of the Royal Society of London.*

nothing but an assemblage of a number of good conductors of a different kind, arranged in a certain manner. Thus, 30, 40, 60, or more pieces of copper, or better of silver, each applied to a piece of tin or still better of zinc, and an equal number of layers of water, or of some other liquid which may be a better conductor than simple water, as salt water, lye, &c., or of bits of card or leather, &c., soaked in such liquids. Of such layers interposed between each couple or combination of two different metals, one such alternate series, and always in the same order, of these three kinds of conductors, is all that constitutes M. Volta's new instrument; which imitates so well the effects of the Leyden phial or electric batteries; not indeed with the force and explosions of these, when highly charged; but only equaling the effects of a battery charged to a very weak degree, of a battery, however, having an immense capacity, but which besides infinitely surpasses the virtue and the power of these same batteries; as it has no need, like them, of being charged beforehand, by means of a foreign electricity; and as it is capable of giving the usual commotion as often as ever it is properly touched. This apparatus, as it resembles more the natural electric organ of the torpedo, or of the electric eel than the Leyden phial and the ordinary electric batteries, M. Volta calls the artificial electric organ. For the construction of this instrument, M. Volta provides some dozens of small round metal plates of copper, or tin, or best of silver, about an inch in diameter, like shillings or half-crowns, and an equal number of plates of tin, or much better of zinc, of the same shape and size. These pieces he places exactly one upon another, forming a column, pillar or pile. He provides also as many round pieces of card, or leather, or such like spongy matter, capable of imbibing and retaining much of the water, or other liquid, when soaked in it. These soaked roulets or circles are to be a little less in diameter than the small metal discs or plates, that they may not jut out beyond them. All these discs are then placed horizontally on a table, one over another continually alternating, in a pile as high as will well support itself without tottering and falling down: beginning with a plate of either of the metals, as for instance, the silver, then upon that one of zinc, over which is to be put the soaked card; then other three discs, over these in the same order, viz. a silver, next a zinc, and then another moistened card, &c.

After having raised the pile to about 20 of these stages or triads of plates, it will be already capable, not only of affecting Cavallo's electrometer, assisted by the condenser, so as to raise it 10 or 15°, charging it by a simple touching, so as to cause it to give a spark, &c., as also to strike the fingers with which we touch the top or bottom of the column, with several small snaps, the fingers being wetted with water. But if to the 20 sets of triplets of the plates be added 20 or 30 more, disposed in the same order, the actions of the extended pile will be much stronger, and be felt through the arms up to the shoulders; and by continuing the touchings, the pains in the hands become insupportable.

M. Volta constructs and combines his apparatus in various ways and forms, more or less powerful, convenient or amusing. One is as follows (Fig. 1, pl. 13.), which he calls a *couronne de tasses*. He disposes in a row a number of cups of wood, or earth, or glass, or any thing but metal, half filled with pure water, or salt water or lye; these are all made to communicate in a kind of chain, by several metallic arcs of which one arm or link, Aa, or only the extremity A, immersed in one of the cups, is of copper, or of copper silvered, and the other Z, immersed in the following cup, is of tin, or rather of zinc, the other two being soldered together near the crown of the arch. It is evident that a series of these cups, thus connected together, either in a straight or curved line, by the two metals and the intermediate liquid, is similar to the pillar or pile before described, and consequently will exhibit similar effects. Thus, to produce commotion or sensation in the hands and arms, we need only dip one hand into one of the cups and the finger of the other hand into another cup, sufficiently far from the former; and the action will be so much the stronger as the two cups are farther asunder, or have the more intermediate cups; and consequently the greatest by touching the first and the last in the chain.

M. Volta concludes with various remarks and cautions in using this instrument; showing that it is perpetual in its virtue, renewing its charge spontaneously, and serving most of the purposes of the ordinary electrical machines, and even affecting and manifesting its power by most of the human senses, viz. feeling, tasting, hearing, and seeing.

XIX

PIERRE SIMON LAPLACE

1749-1827

Pierre Simon Laplace, born at Beaumont-en-Auge, Normandy, March 28, 1749, became a teacher of mathematics at Beaufort before he was eighteen years old. He gained d'Alembert's attention by a letter which he wrote to him on the principles of mathematics. After 1770 he engaged with Lagrange in determining the permanency of the solar system by studying its perturbations and interactions, and finally suggested how these changes were periodic. His monumental work, in five volumes, "Mechanics of the Heavens" (1799-1825), gave a comprehensive description of the movements of the solar system, and his "System of the World" proposed the nebular theory of the origin of the universe. His researches were important in the development of modern astronomy because he substituted a dynamic for the descriptive point of view. He died at Arcueil, March 5, 1827.

THE NEBULAR HYPOTHESIS *

Buffon is the only individual that I know of, who, since the discovery of the true system of the world, endeavoured to investigate the origin of the planets and satellites. He supposed that a comet, by impinging on the Sun, carried away a torrent of matter, which was reunited far off, into globes of different magnitudes and at different distances from this star. These globes, when they cool and become hardened, are the planets and their satellites. This hypothesis sat-

* Translated from *Exposition du Système du Monde*, (Paris, 1796).

ifies the first of the five preceding phenomena * ; for it is evident that all bodies thus formed should move very nearly in the plane which passes through the centre of the Sun, and through the direction of the torrent of matter which has produced them: but the four remaining phenomena appear to me inexplicable on this supposition. Indeed, the absolute motion of the molecules of a planet ought to be in the same direction as the motion of the centre of gravity; but it by no means follows from this, that the motion of rotation of a planet should be also in the same direction. Thus the Earth may revolve from east to west, and yet the absolute motion of each of its molecules may be directed from west to east. This observation applies also to the revolution of the satellites, of which the direction in the same hypothesis, is not necessarily the same as that of the motion of projection of the planets.

The small eccentricity of the planetary orbits is a phenomenon, not only difficult to explain on this hypothesis, but altogether inconsistent with it. We know from the theory of central forces, that if a body which moves in a re-entrant orbit about the Sun, passes very near the body of the Sun, it will return constantly to it, at the end of each revolution. Hence it follows that if the planets were originally detached from the Sun, they would touch it, at each return to this star; and their orbits, instead of being nearly circular, would be very eccentric. Indeed it must be admitted that a torrent of matter detached from the Sun, cannot be compared to a globe which just skims by its surface; from the impulsions which the parts of this torrent receive from each other, combined with their mutual attraction, they may, by changing the direction of their motions, increase the distances of their perihelions from the Sun. But their orbits should be extremely eccentric, or at least all the orbits would not be q. p. circular, except by the most extraordinary chance. Finally, no reason can be assigned on the hypothesis of Buffon, why the orbits of more than one hundred comets, which have been al-

* viz: "The motions of the planets in the same direction, and very nearly in the same plane; the motions of the satellites in the same direction as those of the planets; the motions of the rotation of these different bodies and also of the sun, in the same direction as their motions of projection, and in planes very little inclined to each other; the small eccentricity of the orbits of the comets and satellites; finally, the great eccentricity of the orbits of the comets, their inclinations being at the same time entirely indeterminate."

ready observed, should be all very eccentric. The hypothesis, therefore, is far from satisfying the preceding phenomena. Let us consider whether we can assign the true cause.

Whatever may be its nature, since it has produced or influenced the direction of the planetary motions, it must have embraced them all within the sphere of its action; and considering the immense distance which intervenes between them, nothing could have effected this but a fluid of almost indefinite extent. In order to have impressed on them all a motion *q. p.* circular and in the same direction about the Sun, this fluid must environ this star, like an atmosphere. From a consideration of the planetary motions, we are therefore brought to the conclusion, that in consequence of an excessive heat, the solar atmosphere originally extended beyond the orbits of all the planets, and that it has successively contracted itself within its present limits.

In the primitive state in which we have supposed the Sun to be, it resembles those substances which are termed *nebulæ*, which, when seen through telescopes, appear to be composed of a nucleus, more or less brilliant, surrounded by a nebulosity, which, by condensing on its surface, transforms it into a star. If all the stars are conceived to be similarly formed, we can suppose their anterior state of nebulosity to be preceded by other states, in which the nebulous matter was more or less diffuse, the nucleus being at the same time more or less brilliant. By going back in this manner, we shall arrive at a state of nebulosity so diffuse, that its existence can with difficulty be conceived.

For a considerable time back, the particular arrangement of some stars visible to the naked eye, has engaged the attention of philosophers. Mitchel remarked long since how extremely improbable it was that the stars composing the constellation called the Pleiades, for example, should be confined within the narrow space which contains them, by the sole chance of hazard; from which he inferred that this group of stars, and the similar groups which the heavens present to us, are the effects of a primitive law of nature. These groups are a general result of the condensation of *nebulæ* of several nuclei; for it is evident that the nebulous matter being perpetually attracted by these different nuclei, ought at length to form a group of stars, like to that of the Pleiades. The condensation of *nebulæ* consisting of

two nuclei, will in like manner form stars very near to each other, revolving the one about the other like to the double stars, whose respective motions have been already recognized.

But in what manner has the solar atmosphere determined the motions of rotation and revolution of the planets and satellites? If these bodies had penetrated deeply into this atmosphere, its resistance would cause them to fall on the Sun. We may therefore suppose that the planets were formed at its successive limits, by the condensation of zones of vapours, which it must, while it was cooling, have abandoned in the plane of its equator.

Let us resume the results which we have given in the tenth chapter of the preceding book. The Sun's atmosphere cannot extend indefinitely; its limit is the point where the centrifugal force arising from the motion of rotation balances the gravity; but according as the cooling contracts the atmosphere, and condenses the molecules which are near to it, on the surface of the star, the motion of rotation increases; for, in virtue of the principle of areas, the sum of the areas described by the *radius vector* of each particle of the Sun and its atmosphere, and projected on the plane of its equator, is always the same. Consequently the rotation ought to be quicker, when these particles approach to the centre of the Sun. The centrifugal force arising from this motion becoming thus greater; the point where the gravity is equal to it, is nearer to the centre of the Sun. Supposing, therefore, what is natural to admit, that the atmosphere extended at any epoch as far as this limit, it ought, according as it cooled, to abandon the molecules, which are situated at this limit, and at the successive limits produced by the increased rotation of the Sun. These particles, after being abandoned, have continued to circulate about this star, because their centrifugal force was balanced by their gravity. But as this equality does not obtain for these molecules of the atmosphere which are situated on the parallels to the Sun's equator, these have come nearer by their gravity to the atmosphere according as it condensed, and they have not ceased to belong to it inasmuch as by their motion, they have approached to the plane of this equator.

Let us now consider the zones of vapours, which have been successively abandoned. These zones ought, according to all probability, to form by their condensation, and by the mutual attraction of their

particles, several concentrical rings of vapours circulating about the Sun. But mutual friction of the molecules of each ring ought to accelerate some and retard others, until they all had acquired the same angular motion. Consequently the real velocities of the molecules which are farther from the Sun, ought to be greatest. The following cause ought likewise to contribute to this difference of velocities: The most distant particles of the Sun, and which, by the effects of cooling and condensation, have collected so as to constitute the superior part of the ring, have always described areas proportional to the times, because the central force by which they are actuated has been constantly directed to this star; but this constancy of areas requires an increase of velocity, according as they approach more to each other. It appears that the same cause ought to diminish the velocity of the particles, which, situated near the ring, constitute its inferior part.

If all the particles of a ring of vapours continued to condense without separating, they would at length constitute a solid or a liquid ring. But the regularity which this formation requires in all the parts of the ring, and in their cooling, ought to make this phenomenon very rare. Thus the solar system presents but one example of it; that of the rings of Saturn. Almost always each ring of vapours ought to be divided into several masses, which, being moved with velocities which differ little from each other, should continue to revolve at the same distance about the Sun. These masses should assume a spheroidal form, with a rotatory motion in the direction of that of their revolution, because their inferior particles have a less real velocity than the superior; they have therefore constituted so many planets in a state of vapour. But if one of them was sufficiently powerful, to unite successively by its attraction, all the others about its centre, the ring of vapours would be changed into one sole spheroidal mass, circulating about the Sun, with a motion of rotation in the same direction with that of revolution. This last case has been the most common; however, the solar system presents to us the first case, in the four small planets which revolve between Mars and Jupiter, at least unless we suppose with Olbers, that they originally formed one planet only, which was divided by an explosion into several parts, and actuated by different velocities. Now if we trace the changes which a further cooling ought to produce in the planets

formed of vapours, and of which we have suggested the formation, we shall see to arise in the centre of each of them, a nucleus increasing continually, by the condensation of the atmosphere which environs it. In this state, the planet resembles the Sun in the nebulous state, in which we have first supposed it to be; the cooling should therefore produce at the different limits of its atmosphere, phenomena similar to those which have been described, namely, rings and satellites circulating about its centre in the direction of its motion of rotation, and revolving in the same direction on their axes. The regular distribution of the mass of rings of Saturn about its centre and in the plane of its equator, results naturally from this hypothesis, and, without it, is inexplicable. Those rings appear to me to be existing proofs of the primitive extension of the atmosphere of Saturn, and of its successive condensations. Thus, the singular phenomena of the small eccentricities of the orbits of the planets and satellites, of the small inclination of these orbits to the solar equator, and of the identity in the direction of the motions of rotation and revolution of all those bodies with that of the rotation of the Sun, follow the hypothesis which has been suggested, and render it extremely probable. If the solar system was formed with perfect regularity, the orbits of the bodies which compose it would be circles, of which the planes, as well as those of the various equators and rings, would coincide with the plane of the solar equator. But we may suppose that the innumerable varieties which must necessarily exist in the temperature and density of different parts of these great masses, ought to produce the eccentricities of their orbits, and the deviations of their motions, from the plane of this equator.

In the preceding hypothesis, the comets do not belong to the solar system. If they be considered, as we have done, as small *nebulæ*, wandering from one solar system to another, and formed by the condensation of the nebulous matter, which is diffused so profusely throughout the universe, we may conceive that when they arrive in that part of space where the attraction of the Sun predominates, it should force them to describe elliptic or hyperbolic orbits. But as their velocities are equally possible in every direction, they must move indifferently in all directions, and at every possible inclination to the elliptic; which is conformable to observation. Thus the condensation of the nebulous matter, which explains the motions

of rotation and revolution of the planets and satellites in the same direction, and in orbits very little inclined to each other, likewise explains why the motions of the comets deviate from this general law.

The great eccentricity of the orbits of the comets, is also a result of our hypothesis. If those orbits are elliptic, they are very elongated, since their greater axes are at least equal to the radius of the sphere of activity of the Sun. But these orbits may be hyperbolic; and if the axes of these hyperbolæ are not very great with respect to the mean distance of the Sun from the Earth, the motion of the comets which describe them will appear to be sensibly hyperbolic. However, with respect to the hundred comets, of which the elements are known, not one appears to move in a hyperbola; hence the chances which assign a sensible hyperbola are extremely rare relatively to the contrary chances. The comets are so small, that they only become sensible when their perihelion distance is inconsiderable. Hitherto this distance has not surpassed twice the diameter of the Earth's orbit, and most frequently, it has been less than the radius of this orbit. We may conceive, that in order to approach so near to the Sun, their velocity at the moment of their ingress within its sphere of activity, must have an intensity and direction confined within very narrow limits. If we determine by the analysis of probabilities, the ratio of the chances which in these limits, assign a sensible hyperbola to the chances which assign an orbit, which may without sensible error be confounded with a parabola, it will be found that there is at least six thousand to unity that a nebula which penetrates within the sphere of the Sun's activity so as to be observed, will either describe a very elongated ellipse, or an hyperbola, which, in consequence of the magnitude of its axis will be as to sense confounded with a parabola in the part of its orbit which is observed. It is not therefore surprising that hitherto no hyperbolic motions have been recognized.

The attraction of the planets, and perhaps also the resistance of the ethereal media, ought to change several cometary orbits into ellipses, of which the greater axes are much less than the radius of the sphere of the solar activity. It is probable that such a change was produced in the orbit of the comet of 1759, the greater axis of which was not more than thirty-five times the distance of the Sun from the Earth. A still greater change was produced in the orbits of the comets of 1770 and of 1805.

If in the zones abandoned by the atmosphere of the Sun, there are any molecules too volatile to be united to each other, or to the planets, they ought in their circulation about this star to exhibit all the appearances of the zodiacal light, without opposing any sensible resistance to the different bodies of the planetary system, both on account of their great rarity and also because their motion is very nearly the same as that of the planets which they meet.

An attentive examination of all the circumstances of this system renders our hypothesis still more probable. The primitive fluidity of the planets is clearly indicated by the compression of their figure, conformably to the laws of the mutual attraction of their molecules; it is moreover demonstrated by the regular diminution of gravity, as we proceed from the equator to the poles. This state of primitive fluidity to which we are conducted by astronomical phenomena, is also apparent from those which natural history points out. But in order fully to estimate them, we should take into account the immense variety of combinations formed by all the terrestrial substances which were mixed together in a state of vapour, when the depression of their temperature enabled their elements to unite; it is necessary likewise to consider the wonderful changes which this depression ought to cause in the interior and at the surface of the earth, in all its productions, in the constitution and pressure of the atmosphere, in the ocean, and in all substances which it held in a state of solution. Finally, we should take into account the sudden changes, such as great volcanic eruptions, which must at different epochs have deranged the regularity of these changes. Geology, thus studied under the point of view which connects it with astronomy, may, with respect to several objects, acquire both precision and certainty.

One of the most remarkable phenomena of the solar system is the rigorous equality which is observed to subsist between the angular motions of rotation and revolution of each satellite. It is infinity to unity that this is not the effect of hazard. The theory of universal gravitation makes infinity to disappear from this improbability, by shewing that it is sufficient for the existence of this phenomenon, that at the commencement these motions did not differ much. Then, the attraction of the planet would establish between them a perfect equality; but at the same time it has given rise to a periodic oscillation in the axis of the satellite directed to the planet, of which oscilla-

tion the extent depends on the primitive difference between these motions. As the observations of Mayer on the libration of the Moon, and those which Bouvard and Nicollet made for the same purpose, at my request, did not enable us to recognize this oscillation; the difference on which it depends must be extremely small, which indicates with every appearance of probability the existence of a particular cause, which has confined this difference within very narrow limits, in which the attraction of the planet might establish between the mean motions of rotation and revolution a rigid equality, which at length terminated by annihilating the oscillation which arose from this equality. Both these effects result from our hypothesis; for we may conceive that the Moon, in a state of vapour, assumed in consequence of the powerful attraction of the earth the form of an elongated spheroid, of which the greater axis would be constantly directed towards this planet, from the facility with which the vapours yield to the slightest force impressed upon them. The terrestrial attraction continuing to act in the same manner, while the Moon is in a state of fluidity, ought at length, by making the two motions of this satellite to approach each other, to cause their difference to fall within the limits, at which their rigorous equality commences to establish itself. Then this attraction should annihilate, by little and little, the oscillation which this equality produced on the greater axis of the spheroid directed towards the earth. It is in this manner that the fluids which cover this planet, have destroyed by their friction and resistance the primitive oscillations of its axis of rotation, which is only now subject to the nutation resulting from the actions of the Sun and Moon. It is easy to be assured that the equality of the motions of rotation and revolution of the satellites ought to oppose the formation of rings and secondary satellites, by the atmospheres of these bodies. Consequently observation has not hitherto indicated the existence of any such. The motions of the three first satellites of Jupiter present a phenomenon still more extraordinary than the preceding; which consists in this, that the mean longitude of the first, minus three times that of the second, plus twice that of the third, is constantly equal to two right angles. There is the ratio of infinity to one, that this equality is not the effect of chance. But we have seen, that in order to produce it, it is sufficient if at the commencement, the mean motions of these three bodies ap-

proached very near to the relation which renders the mean motion of the first, minus three times that of the second, plus twice that of the third, equal to nothing. Then their mutual attraction rendered this ratio rigorously exact, and it has moreover made the mean longitude of the first minus three times that of the second, plus twice that of the third, equal to a semicircumference. At the same time, it gave rise to a periodic inequality, which depends on the small quantity, by which the mean motions originally deviated from the relation which we have just announced. Notwithstanding all the care Delambre took in his observations, he could not recognize this inequality, which, while it evinces its extreme smallness, also indicates, with a high degree of probability, the existence of a cause which makes it to disappear. In our hypothesis, the satellites of Jupiter, immediately after their formation, did not move in a perfect vacuo; the less condensable molecules of the primitive atmospheres of the Sun and planet would then constitute a rare medium, the resistance of which being different for each of the stars, might make the mean motions to approach by degrees to the ratio in question; and when these movements had thus attained the conditions requisite, in order that the mutual attraction of the three satellites might render this relation accurately true, it perpetually diminished the inequality which this relation originated, and eventually rendered it insensible. We cannot better illustrate these effects than by comparing them to the motion of a pendulum, which, actuated by a great velocity, moves in a medium, the resistance of which is inconsiderable. It will first describe a great number of circumstances; but at length its motion of circulation perpetually decreasing, it will be converted into an oscillatory motion, which itself diminishing more and more, by the resistance of the medium, will eventually be totally destroyed, and then the pendulum, having attained a state of repose, will remain at rest for ever.

XX

EDWARD JENNER

1749-1823

Edward Jenner, born May 17, 1749, at Berkeley, Gloucestershire, England, studied surgery under John Hunter at London, and returned to his native town to practise. Having learned, about 1796, that milkmaids who had caught the cowpox were immune from smallpox, he began at once to make investigations and to conduct experiments. This led to his "Inquiry," published in 1798, in which he made public his theory of vaccination. His discovery created widespread interest, but although the theory at once met with the most virulent criticism, vaccination was soon widely accepted. By 1801, ten thousand persons were vaccinated in England, and the beneficent results justified its wide adoption. He died of apoplexy, January 26, 1823.

THE THEORY OF VACCINATION *

The deviation of Man from the state in which he was originally placed by Nature seems to have proved to him a prolific source of Diseases. From the love of splendour, from the indulgences of luxury, and from his fondness for amusement, he has familiarised himself with a great number of animals, which may not originally have been intended for his associates.

The Wolf, disarmed of ferocity, is now pillowed in the lady's lap. The Cat, the little Tyger of our island, whose natural home is the forest, is equally domesticated and caressed. The Cow, the Hog, the Sheep, and the Horse, are all, for a variety of purposes, brought under his care and dominion.

There is a disease to which the Horse, from his state of domestica-

* From *An Inquiry into the Cause and Effects of the Variolae Vaccinae.*

tion, is frequently subject. The Farriers have termed it the Grease. It is an inflammation and swelling in the heel, from which issues matter possessing properties of a very peculiar kind, which seems capable of generating a disease in the Human Body (after it has undergone the modification which I shall presently speak of), which bears so strong a resemblance to the Small-pox that I think it highly probable it may be the source of that disease.

In this Dairy Country a great number of Cows are kept, and the office of milking is performed indiscriminately by Men and Maid Servants. One of the former having been appointed to apply dressings to the heels of a Horse affected with the Grease, and not paying due attention to cleanliness, incautiously bears his part in milking the Cows, with some particles of the infectious matter adhering to his fingers. When this is the case, it commonly happens that a disease is communicated to the Cows, and from the Cows to the Dairy-maids, which spreads through the farm until most of the cattle and domestics feel its unpleasant consequences. This disease has obtained the name of the Cow-pox. It appears on the nipples of the Cows in the form of irregular pustules. At their first appearance they are commonly of a palish blue, or rather of a colour somewhat approaching to livid, and are surrounded by an erysipelatous inflammation. These pustules, unless a timely remedy be applied, frequently degenerate into phagedenic ulcers, which prove extremely troublesome. The animals become indisposed, and the secretion of milk is much lessened. Inflamed spots now begin to appear on different parts of the hands of the domestics employed in milking, and sometimes on the wrists, which quickly run on to suppuration, first assuming the appearance of the small vesications produced by a burn. Most commonly they appear about the joints of the fingers, and at their extremities; but whatever parts are affected, if the situation will admit, these superficial suppurations put on a circular form, with their edges more elevated than their centre, and of a colour distantly approaching to blue. Absorption takes place, and tumours appear in each axilla. The system becomes affected—the pulse is quickened; and shiverings, with general lassitude and pains about the loins and limbs, with vomiting, come on. The head is painful, and the patient is now and then even affected with delirium. These symptoms, varying in their degrees of violence, generally continue from one day to three or four,

leaving ulcerated sores about the hands, which, from the sensibility of the parts, are very troublesome, and commonly heal slowly, frequently becoming phagedenic, like those from whence they sprung. The lips, nostrils, eyelids, and other parts of the body, are sometimes affected with sores; but these evidently arise from their being needlessly rubbed or scratched with the patient's infected fingers. No eruptions on the skin have followed the decline of the feverish symptoms in any instance that has come under my inspection, one only excepted, and in this case a very few appeared on the arms: they were very minute, of a vivid red colour, and soon died away without advancing to maturation; so that I cannot determine whether they had any connection with the preceding symptoms.

Thus the disease makes its progress from the Horse to the nipple of the Cow, and from the Cow to the Human Subject.

Morbid matter of various kinds, when absorbed into the system, may produce effects in some degree similar; but what renders the Cow-pox virus so extremely singular is, that the person who has been thus affected is forever after secure from the infection of the Small-pox; neither exposure to the *variolous effluvia*, nor the insertion of the matter into the skin producing this distemper.

[I shall now conclude this Inquiry with some general observations on the subject, and on some others which are interwoven with it.]

Although I presume it may be unnecessary to produce further testimony in support of my assertion "that Cow-pox protects the human constitution from the infection of the Small-pox," yet it affords me considerable satisfaction to say that Lord Somerville, the president of the Board of Agriculture, to whom this paper was shown by Sir Joseph Banks, has found upon inquiry that the statements were confirmed by the concurring testimony of Mr. Dolland, a surgeon, who resides in a dairy country remote from this, in which these observations were made. With respect to the opinion adduced "that the source of the infection is a peculiar morbid matter arising in the horse," although I have not been able to prove it from actual experiments conducted immediately under my own eye, yet the evidence I have adduced appears sufficient to establish it.

They who are not in the habit of conducting experiments may not be aware of the coincidence of circumstances necessary for their being managed so as to prove perfectly decisive; nor how often men engaged in professional pursuits are liable to interruptions which dis-appoint them almost at the instant of their being accomplished.

[However, I feel no room for hesitation respecting the common origin of the disease, being well convinced that it never appears among the cows (except it can be traced to a cow introduced among the general herd which has been previously infected, or to an infected servant), unless they have been milked by someone who, at the same time, has the care of a horse affected with diseased heels.

The spring of 1797, which I intended particularly to have devoted to the completion of this investigation, proved, from its dryness, remarkably adverse to my wishes; for it frequently happens, while the farmers' horses are exposed to the cold rains which fall at that season that their heels become diseased, and no Cow-pox then appeared in the neighbourhood.]

The active quality of the virus from the horses' heels is greatly increased after it has acted on the nipples of the cow, as it rarely happens that the horse affects his dresser with sores, and as rarely that a milk-maid escapes the infection when she milks infected cows. It is most active at the commencement of the disease, even before it has acquired a pus-like appearance; indeed I am not confident whether this property in the matter does not entirely cease as soon as it is secreted in the form of pus. I am induced to think it does cease, and that it is the thin darkish-looking fluid only, oozing from the newly-formed cracks in the heels, similar to what sometimes appears from erysipelatous blisters, which gives the disease. Nor am I certain that the nipples of the cows are at all times in a state to receive the infection. The appearance of the disease in the spring and the early part of the summer, when they are disposed to be affected with spontaneous eruptions so much more frequently than at other seasons, induces me to think that the virus from the horse must be received upon them when they are in this state, in order to produce effects; experiments, however, must determine these points. But it is clear that when the Cow-pox virus is once generated, that the cows cannot

resist the contagion, in whatever state their nipples may chance to be, if they are milked with an infected hand.

Whether the matter, either from the cow or the horse, will affect the sound skin of the human body, I cannot positively determine; probably it will not, unless on those parts where the cuticle is extremely thin, as on the lips for example. I have known an instance of a poor girl who produced an ulceration on her lip by frequently holding her finger to her mouth to cool the raging of a Cow-pox sore by blowing upon it. The hands of the farmers' servants here, from the nature of their employments, are constantly exposed to those injuries which occasion abrasions of the cuticle, to punctures from thorns and such like accidents; so that they are always in a state to feel the consequences of exposure to infectious matter.

[It is singular to observe that the Cow-pox virus, although it renders the constitution unsusceptible of the variolous, should, nevertheless, leave it unchanged with respect to its own action. I have already produced an instance to point out this, and shall now corroborate it with another.

Elizabeth Wynne, who had the Cow-pox in the year 1759, was inoculated with variolous matter, without effect, in the year 1797, and again caught the Cow-pox in the year 1798. When I saw her, which was on the 8th day after she received the infection, I found her infected with general lassitude, shiverings, alternating with heat, coldness of the extremities, and a quick and irregular pulse. These symptoms were preceded by a pain in the axilla.

It is curious also to observe that the virus, which with respect to its effects is undetermined and uncertain previously to its passing from the horse through the medium of the cow, should then not only become more active, but should invariably and completely possess those specific properties which induce in the human constitution symptoms similar to those of the variolous fever, and effect in it that peculiar change which forever renders it unsusceptible of the variolous contagion.

May it not then be reasonably conjectured that the source of the Small-pox is morbid matter of a peculiar kind, generated by a disease in the horse, and that accidental circumstances may have again and again arisen, still working new changes upon it, until it has acquired the contagious and malignant form under which we now commonly see it making its devastations amongst us? And, from a

consideration of the change which the infectious matter undergoes from producing a disease on the cow, may we not conceive that many contagious diseases, now prevalent among us, may owe their present appearance not to a simple, but to a compound origin? For example, is it difficult to imagine that the measles, scarlet fever, and the ulcerous sore throat with a spotted skin, have all sprung from the same source, assuming some variety in their forms according to the nature of their new combinations? The same question will apply respecting the origin of many other contagious diseases, which bear a strong analogy to each other.

There are certainly more forms than one, without considering the common variation between the confluent and distinct, in which the Small-pox appears in what is called the natural way. About seven years ago a species of Small-pox spread through many of the towns and villages of this part of Gloucestershire: it was of so mild a nature that a fatal instance was scarcely ever heard of, and consequently so little dreaded by the lower orders of the community that they scrupled not to hold the same intercourse with each other as if no infectious disease had been present among them. I never saw nor heard of an instance of its being confluent. The most accurate manner, perhaps, in which I can convey an idea of it, is, by saying that had fifty individuals been taken promiscuously and infected by exposure to this contagion, they would have had as mild and light a disease as if they had been inoculated with variolous matter in the usual way. The harmless manner in which it showed itself could not arise from any peculiarity either in the season or the weather, for I watched its progress upwards of a year without perceiving any variation in its general appearance. I consider it then as a variety of the Small-pox.

[In some of the preceding cases I have noticed the attention that was paid to the state of the variolous matter previous to the experiment of inserting it into the arms of those who had gone through the Cow-pox. This I conceived to be of great importance in conducting these experiments, and were it always properly attended to by those who inoculate for the Small-pox, it might prevent much subsequent mischief and confusion. With the view of enforcing so necessary a precaution, I shall take the liberty of digressing so far as to point out some unpleasant facts relative to mismanagement in this particular, which have fallen under my own observation.]

A medical gentleman (now no more), who for many years inoculated in this neighbourhood, frequently preserved the variolous matter intended for his use, on a piece of lint or cotton, which, in its fluid state, was put into a vial, corked, and conveyed into a warm pocket; a situation certainly favourable for speedily producing putrefaction in it. In this state (not infrequently after it had been taken several days from the pustules) it was inserted into the arms of his patients, and brought on inflammation of the incised parts, swellings of the axillary glands, fever, and sometimes eruptions. But what was this disease? Certainly not the Small-pox; for the matter having from putrefaction lost, or suffered a derangement in its specific properties, was no longer capable of producing that malady, those who had been inoculated in this manner being as much subject to the contagion of the Small-pox, as if they had never been under the influence of this artificial disease; and many, unfortunately, fell victims to it, who thought themselves in perfect security. The same unfortunate circumstance of giving a disease, supposed to be the Small-pox, with inefficacious variolous matter, having occurred under the direction of some other practitioners within my knowledge, and probably from the same incautious method of securing the variolous matter, I avail myself of this opportunity of mentioning what I conceive to be of great importance; and, as a further cautionary hint, I shall again digress so far as to add another observation on the subject of Inoculation.

Whether it be yet ascertained by experiment, that the quantity of variolous matter inserted into the skin makes any difference with respect to the subsequent mildness or violence of the disease, I know not; but I have the strongest reason for supposing that if either the punctures or incisions be made so deep as to go through it, and wound the adipose membrane, that the risk of bringing on a violent disease is greatly increased. I have known an inoculator, whose practice was "to cut deep enough (to use his own expression) to see a bit of fat," and there to lodge the matter. The great number of bad cases, independent of inflammations and abscesses on the arms, and the fatality which attended this practice was almost inconceivable; and I cannot account for it on any other principle than that of the matter being placed in this situation instead of the skin.

At what period the Cow-pox was first noticed here is not upon

record. Our oldest farmers were not unacquainted with it in their earliest days, when it appeared among their farms without any deviation from the phenomena which it now exhibits. Its connection with the Small-pox seems to have been unknown to them. Probably the general introduction of inoculation first occasioned the discovery.

Its rise in this country may not have been of very remote date, as the practice of milking cows might formerly have been in the hands of women only; which I believe is the case now in some other dairy countries, and consequently that the cows might not in former times have been exposed to the contagious matter brought by the men servants from the heels of horses. Indeed a knowledge of the source of the infection is new in the minds of most of the farmers in this neighbourhood, but it has at length produced good consequences; and it seems probable from the precautions they are now disposed to adopt, that the appearance of the Cow-pox here may either be entirely extinguished or become extremely rare.

Should it be asked whether this investigation is a matter of mere curiosity, or whether it tends to any beneficial purpose, I should answer that, notwithstanding the happy effects of inoculation, with all the improvements which the practice has received since its first introduction into this country, it not very infrequently produces deformity of the skin, and sometimes, under the best management, proves fatal.

These circumstances must naturally create in every instance some degree of painful solicitude for its consequences. But as I have never known fatal effects arise from the Cow-pox, even when impressed in the most unfavourable manner, producing extensive inflammations and suppurations on the hands; and as it clearly appears that this disease leaves the constitution in a state of perfect security from the infection of the Small-pox, may we not infer that a mode of inoculation may be introduced preferable to that at present adopted, especially among those families which, from previous circumstances, we may judge to be predisposed to have the disease unfavourably? It is an excess in the number of pustules which we chiefly dread in the Small-pox; but, in the Cow-pox, no pustules appear, nor does it seem possible for the contagious matter to produce the disease from effluvia, or by any other means than contact, and that probably not simply between the virus and the cuticle; so that a single individual in a family might at any

time receive it without the risk of infecting the rest, or of spreading a distemper that fills a country with terror.

[Several instances have come under my observation which justify the assertion that the disease cannot be propagated by effluvia. The first boy whom I inoculated with the matter of Cow-pox slept in a bed while the experiment was going forward, with two children who had never gone through either that disease or the Small-pox, without infecting either of them.

A young woman who had the Cow-pox to a great extent, several sores which matured having appeared on the hands and wrists, slept in the same bed with a fellow-dairymaid, who never had been infected with either the Cow-pox or the Small-pox, but no indisposition followed.

Another instance has occurred of a young woman on whose hands were several large suppurations from the Cow-pox, who was at the same time a daily nurse to an infant, but the complaint was not communicated to the child.]

In some other points of view the inoculation of this disease appears preferable to the variolous inoculation.

In constitutions predisposed to scrofula, how frequently we see the inoculated Small-pox rouse into activity that distressful malady. This circumstance does not seem to depend on the manner in which the distemper has shown itself, for it has as frequently happened among those who have had it mildly, as when it has appeared in the contrary way. There are many, who from some peculiarity in the habit resist the common effects of variolous matter inserted into the skin, and who are in consequence haunted through life with the distressing idea of being insecure from subsequent infection. A ready mode of dissipating anxiety originating from such a cause must now appear obvious. And, as we have seen that the constitution may at any time be made to feel the fertile attack of Cow-pox, might it not, in many chronic diseases, be introduced into the system, with the probability of affording relief, upon well-known physiological principles?

Although I say the system may at any time be made to feel the febrile attack of Cow-pox, yet I have a single instance before me where the virus acted locally only, but it is not in the least probable that the same person would resist the action both of Cow-pox virus and the variolous.

XXI
COUNT RUMFORD

1753-1814

Sir Benjamin Thompson, Count Rumford, was born in Woburn, Massachusetts, March 26, 1753, a member of an old New England family. After a very romantic youth and early manhood in which he underwent many exciting adventures as a British loyalist at the time of the American Revolution, he was sent to England with despatches by the British expeditionary authorities and there found employment in the office of the Secretary of State. After the close of the Revolution he went to Bavaria, where he became Minister of War and Grand Chamberlain. In 1791 he was made a count of the Holy Roman Empire. In 1796 President Adams invited him to return to America to become an inspector of artillery, but he declined; and at about the same time he became interested in problems of heat, light, and fuel. His suggestions ultimately became the basis for the doctrine of the conservation of energy. He died at Auteuil, August 25, 1814.

THE NATURE OF HEAT *

After I had long meditated upon a way of putting this interesting problem entirely out of doubt by a perfectly conclusive experiment, I thought finally that I had discovered it, and I think so still.

I argued that if the existence of caloric was a fact, it must be absolutely impossible for a body or for several individual bodies, which together made one whole, to communicate this substance continuously to various other bodies by which they were surrounded, without this substance gradually being entirely exhausted.

* From *An Enquiry Concerning the Source of Heat Excited by Friction* (1798)—*Transactions of the Royal Society of London*.

A sponge filled with water, and hung by a thread in the middle of a room filled with dry air, communicates its moisture to the air, it is true, but soon the water evaporates and the sponge can no longer give out moisture. On the contrary, a bell sounds without interruption when it is struck, and gives out its sound as often as we please without the slightest perceptible loss. Moisture is a substance; sound is not.

It is well known that two hard bodies, if rubbed together, produce much heat. Can they continue to produce it without finally becoming exhausted? Let the result of experiment decide this question.

It would be too tedious to describe here in detail all the experiments which I undertook with a view of answering in a decisive manner this important and disputed question. They may be found in my memoir, "On the Source of Heat excited by Friction." I have had it printed in the *Philosophical Transactions* for the year 1798; still these experiments bear too close a relation to my later researches on heat for me to omit attempting at least to give the reader a clear idea of the experiments and of their results.

The apparatus which I used in these investigations is too complicated to be represented in this place; still it will not be difficult for the reader to form a conception of the principal experiments and their results.

Let A be the vertical section of a brass rod which is an inch in diameter and is fastened in an upright position on a stout block, B; it is provided at its upper end with a massive hemisphere of the same metal, three and a half inches in diameter. C is a similar rod, likewise vertical, to the lower end of which is fastened a similar hemisphere. Both hemispheres must fit each other in such a way that both the rods stand in a perfectly straight vertical line.

D is the vertical section of a globular metallic vessel twelve inches in diameter, which is provided with a cylindrical neck three inches long and three and three-quarter inches in diameter. The rod A goes through a hole in the bottom of the vessel, is soldered into the vessel, and serves as a support to keep it in its proper position.

The centre of the ball, made up of the two hemispheres which lie the one upon the other, is in the centre of the globular vessel, so that, if the vessel is filled with water, the water covers the ball as well as a part of each of the brass rods.

If now the hemispheres be pressed strongly together, and at the same time the rod C be turned, by some means or other, about its axis, a very considerable quantity of heat is generated by means of the friction which takes place between the flat surfaces of the two hemispheres.

The quantity of the heat excited in this manner is exactly proportional to the force with which the two surfaces are pressed together, and to the rapidity of the friction. When this force was equal to the pressure of ten thousand pounds, and when the rod was turned with such rapidity about its axis that it revolved thirty-two times a minute, the quantity of heat generated by the continual rubbing of the two surfaces together was extraordinarily great. It was equal to the quantity given off by the flame of nine wax-candles of moderate size all burning together.

The quantity of heat generated in this manner during a given time is manifestly the same, whether the globular vessel D is filled with water, and the surfaces of the two hemispheres rub on each other in this liquid, or whether there is no water in the vessel, and the apparatus by which the friction is produced is simply surrounded by air.

The source of the heat which is generated by this apparatus is inexhaustible. As long as the rod C is turned about its axis, so long will heat be produced by the apparatus, and always to the same amount.

If the globe-shaped vessel D is filled with water, this water becomes hotter and hotter, and finally begins to boil. I have myself in this way boiled a considerable quantity of water.

If this experiment is performed in winter when the temperature of the air is but little above the freezing-point, and if the vessel D is filled with a mixture of water and pounded ice, the quantity of heat caused in a given time by the rubbing together of the two surfaces can be expressed very exactly by the amount of ice melted by this heat.

Since the apparatus affords heat continuously, and always to the same amount, we can melt in this way as much ice as we please.

But whence comes this heat? This is the contested point, to determine which was the real aim of the experiment.

It is certain that it comes neither from the decomposition of the water nor from the decomposition of the air. Various experiments on

this point, which I have described at length in my memoir in the *Philosophical Transactions*, are more than sufficient to establish this fact beyond doubt.

Just as little does it come from a change in the capacity for heat brought about by friction in the metal of which the hemispheres are composed. This is shown, first, by the continuance and uniformity of the production of the heat; and, secondly, by an experiment bearing directly on this point, by which I am convinced that not the slightest change had taken place in the capacity of the metal for heat.

Just as little does it come from the rods which are attached to the hemispheres, for these rods were always warm, the hemispheres communicating heat to them.

Much less could this heat come from the air of the water immediately surrounding the hemispheres, for the apparatus communicated heat to both these fluids without cessation.

Whence, then, came this heat? and what is heat actually?

I must confess that it has always been impossible for me to explain the results of such experiments except by taking refuge in the very old doctrine which rests on the supposition that heat is nothing but a vibratory motion taking place among the particles of bodies.

A bell, on being struck, immediately gives forth a sound, and the oscillations of the air produced by these vibrations forthwith cause a quivering motion in those bodies with which they come in contact. On the other hand, a sponge filled with water cannot give off its moisture to the bodies in its vicinity for any length of time without itself losing moisture.

A very illustrious philosopher, for whom I have always entertained the greatest respect, and whom, moreover, I have the good fortune to count among my most intimate friends, M. Bertholet, has, in his admirable *Essai de Statique Chimique*, attempted to explain the results of this investigation, and to reconcile them with that theory of heat which is founded upon the hypothesis of caloric.

If a man as learned, as honest, as worthy, and as renowned as is M. Bertholet spares no pains in opposing the errors of a natural philosopher or chemist, one cannot and dare not keep silence unless he wishes to acknowledge himself vanquished. If, however, one can produce proofs—a fortunate thing for all those who find themselves driven to similar self-vindication—that the objections of M. Bertholet

have no foundation, he has done very much towards establishing beyond doubt the opinions and facts in question.

I will now endeavor to answer the objections which M. Bertholet has offered to my explanation of the above-mentioned experiments; and, that the reader may be in a position to give to these objections their just value, I will insert them here in the writer's own words.

"Count Rumford has made a curious experiment with regard to the heat which may be excited by friction. He causes a blunt borer to revolve very rapidly (this borer revolved about its axis only thirty-two times a minute) in a brass cylinder weighing thirteen pounds, English weight (the cylinder weighed one hundred and thirteen pounds and somewhat more), and says that he observed that this borer in the course of two (one and a half) hours, and under a pressure equal to 100 cwt., reduced to powder 4145 grains ($8\frac{1}{2}$ ounces Troy) of brass, and that an amount of heat was generated during this operation sufficient to bring to boil 26.38 pounds of water, previously cooled to the freezing-point. He asserts that he did not discover the slightest difference between the specific heat of the metallic dust and that of the brass which had not experienced the friction. Hence he supposes that the heat was excited by the pressure alone, and was not at all due to caloric, as is the opinion of most chemists.

"I will for the present satisfy myself with simply inquiring whether it necessarily follows from this experiment that we must renounce entirely the received theory of caloric, according to which it is regarded as a substance which enters into combination with bodies, or whether this result cannot be explained in a satisfactory manner by applying to the case in question those laws of nature in accordance with which the operations of heat are manifested under other conditions.

"If the evolution of heat be regarded as a consequence of the decrease of volume caused by the pressure, then not only the metallic powder, but also all the rest of the brass cylinder must have contributed, though not in an equal manner, to this evolution, by the powerful expansive effort of that portion which experienced the greatest pressure, and consequently acquired the greatest temperature, without being able to assume the dimensions proper to this same temperature on account of the less heated and less expanded parts; consequently there must have arisen, necessarily, a certain condensation of the metal in respect of its natural dimensions, which condensation gradually decreased from the point where the pressure was greatest to the surface. We may suppose

that this operation took place in a similar manner in all parts of the cylinder.

"As a consequence of this decrease of volume, an amount of caloric was given out equal to that which would have caused a similar increase of volume, on the supposition, that is, that the specific heat of the metal does not change through this range of the scale of the thermometer, and that the expansions are equal; and this, considering the range of temperatures and the consequent expansions, is probably not far from the truth. The entire amount of heat disengaged would have raised the cylinder to about 180° of Reaumur's scale; and if the expansion of brass by heat is equal to that of iron, which has been found to be 1-75000 for each degree of the thermometer, the 180 degrees would have caused an expansion of 18-75000 in each direction, and the decrease of volume must have brought about the same degree of heat if we suppose that the pressure stood in equal relation to this expansion.

"Now there is a change, and sometimes a very considerable one, wrought in the specific gravity of a metal, by percussion, by the action of a fly-wheel, or by the compression of a wire-drawing machine. It appears, for example, that the specific gravity of platina and of iron, on being forged, is thus increased by a twentieth part.

"Hence it appears that the experiment of Count Rumford is far from explaining satisfactorily a property which is well known, and called in question by no one.

"It is easy, it is true, to arrange side by side in an imposing manner the phenomena of heat; if, however, you were to say to one who has little or no knowledge of chemical speculations, 'Count Rumford's cylinder has, in the course of two hours, by means of a violent friction, afforded all the heat required to dissolve in water, without changing its temperature, 15 kilogrammes of ice, or as much as 2 hectogrammes ($6\frac{1}{2}$ ounces) of oxygen would require [*sic*] in its combination with phosphorus,' I do not know at which of these phenomena he would be most astonished.

"The slight changes which can take place in the amount of combined caloric have so inconsiderable an influence on the capacity for work of the caloric within the narrow limits of the thermometric scale, that it cannot be computed. Moreover, we have not, as yet, adequate data for determining the nature of the changes in this respect which take place in a solid body in consequence of the particular condition of condensation into which it has been brought by means of certain mechanical force, and by degrees of heat differing greatly from each other.

"Besides, Rumford, in the experiment to determine the specific heat of the filings of bell-metal thus obtained, heated them to the temperature

of boiling water. But this extremely elastic heat would very naturally as soon as left to itself, and especially during the operation just mentioned, resume that state of expansion and that capacity for heat which is proper to it at a given temperature, so that the effect of the pressure to which it has been subjected partly disappears again, just as a piece of metal which has been hammered resumes its natural properties on being annealed."

In reply to these remarks, I will call to mind what follows.

1st. The discovery which I made, that no considerable change had taken place in the specific heat of the metallic dust produced by the friction, led me in no way to the supposition that the heat excited in the experiment could not come from the caloric set free. I only found that the source of this heat was inexhaustible. To explain this phenomenon, which has never yet been explained, is the point now in question, and I do not see how it can be explained except by giving up altogether the hypothesis adopted in regard to caloric.

2d. If we actually suppose (and it is far from having been proved) that the simple pressing together of a metal is sufficient to expel the caloric contained in it; still the explanation of such a natural phenomenon would be advanced little or none; for since the action of the force which causes the pressure is continuous, the condensation of the metal brought about by this force would in a short time reach its maximum; and if really in this operation ever so much caloric had been disengaged from the metal, still it would very soon disperse. The rubbing surfaces, on the contrary, continue to give forth heat, and that always to the same amount.

3d. In regard to the objection made to the experiment which was undertaken with a view of determining whether a change had taken place in the capacity of the metallic dust for heat, this can very readily be answered, and in such a way that nothing, it seems to me, can be said against it. If the temperature of boiling water were really sufficient to give to these small, forcibly condensed particles of metal the quantity of heat necessary to bring them back to their original condition as far as their capacity for heat is concerned, then, as the water by which the apparatus was surrounded finally began to boil, they must, without doubt, have taken the necessary amount of heat from this water. If, now, these particles of metal received finally from the water the caloric which in the beginning they imparted to it, the ques-

tion arises, whence came the caloric which served to heat, not only the water, but also the metal and the objects immediately surrounding it?

I am far from desiring to deceive anyone by an imposing arrangement of facts; but the facts in my experiments were so very striking that it was altogether impossible for me to help instituting comparisons and making calculations with regard to them which would make them clear, especially to those not yet sufficiently acquainted with such investigations.

I will now close my remarks with an entirely new computation. I will show whether it is probable that the metal could supply all the heat which was produced by friction in the experiment in question. If we are to make this supposition, we must, in the first place, allow that all the heat came directly from the particles of metal which were separated from the solid mass of metal by the friction; for, since the mass remained in the same condition throughout the entire experiment, it is evident that it could contribute in no measure to the effect produced.

We will now inquire how much heat would have been developed if the experiment had been carried on without cessation, until the whole mass of metal had been reduced to powder by the friction.

After the experiment had lasted an hour and a half, there were 4145 grains (Troy) of the metallic dust, and during that time an amount of heat was produced by the friction sufficient to raise 26.58 pounds of ice-cold water to the boiling point.

Since the mass of metal weighed 113.13 pounds, or 791,190 grains, all this metal would have been reduced to powder if the experiment had lasted uninterruptedly, day and night, for $477\frac{1}{2}$ hours, or for 19 days $21\frac{1}{2}$ hours, and during this time an amount of heat would have been produced sufficient to have raised 5078 pounds of water to the boiling-point.

Since the metal used in this experiment showed a capacity for heat which was to that of water as 0.11 to 1, it is evident that this amount of heat would have been sufficient to raise a mass of the same metal 46,165 pounds in weight through 180 degrees of Fahrenheit's scale, or from the temperature of melting ice to that of boiling water.

This amount of heat would be sufficient to melt a mass of metal sixteen times heavier than that which I used in the experiment.

Is it at all conceivable that such an enormous quantity of caloric

could really be present in this body? But even this supposition would be by no means sufficient for the explanation of the fact in question, as I have shown by a decisive experiment that the capacity of the metal for heat has not sensibly altered.

Whence, then, came the caloric which the apparatus furnished in such abundance?

I leave this question to be answered by those persons who believe in the actual existence of caloric.

In my opinion, I have made it sufficiently evident that it was impossible for it to come from the metallic bodies which were rubbed together, and I am absolutely unable to imagine how it can have come from any other object in the neighborhood of the apparatus, for all these objects received their heat constantly from the apparatus itself.

XXII

JOHN DALTON

1766-1844

John Dalton, son of a weaver, was born in Cumberland, England, September 5, 1766. After an early life spent in teaching in elementary schools, in 1793 he became a teacher of mathematics and philosophy at New College, Manchester. He began his researches into the combination of gases in 1800 and discovered that gases expanded equally with the same pressure and heat. He announced his discovery in a paper read before the Manchester Society in 1801. From further experiments he derived his theory that gases combined with one another in definite proportions, and evolved his atomic theory to explain the results. Awarded the King's medal in 1822, he was further honored by a pension granted in 1833. He died May 27, 1844.

THE ATOMIC THEORY *

There are three distinctions in the kinds of bodies, or three states, which have more especially claimed the attention of philosophical chemists; namely, those which are marked by the terms elastic fluids, liquids, and solids. A very familiar instance is exhibited to us in water, of a body which, in certain circumstances, is capable of assuming all the three states. In steam we recognize a perfectly elastic fluid, in water a perfect liquid, and in ice a complete solid. These observations have tacitly led to the conclusion which seems universally adopted, that all bodies of sensible magnitude, whether liquid or solid, are constituted of a vast number of extremely small particles, or atoms

* From a note entitled *On the Constitution of Bodies* which Dalton wrote and had incorporated in Thomas Thompson's *System of Chemistry* (3d edition, 1807).

of matter bound together by a force of attraction, which is more or less powerful according to circumstances, and which as it endeavours to prevent their separation, is very properly called in that view, attraction of cohesion; but as it collects them from a dispersed state (as from steam into water) it is called attraction of aggregation, or more simply, affinity. Whatever names it may go by, they will signify one and the same power. It is not my design to call in question this conclusion, which appears completely satisfactory; but to show that we have hitherto made no use of it, and that the consequence of the neglect has been a very obscure view of chemical agency, which is daily growing more so in proportion to the new lights attempted to be thrown upon it.

The opinions I more particularly allude to, are those of Bertholet on the Laws of chemical affinity; such as that chemical agency is proportional to the mass, and that in all chemical unions there exist insensible gradations in the proportions of the constituent principles. The inconsistency of these opinions, both with reason and observation, cannot, I think, fail to strike every one who takes a proper view of the phenomena.

Whether the ultimate particles of a body, such as water, are all alike, that is, of the same figure, weight, etc., is a question of some importance. From what is known, we have no reason to apprehend a diversity in these particulars: if it does exist in water, it must equally exist in the elements constituting water, namely, hydrogen and oxygen. Now it is scarcely possible to conceive how the aggregates of dissimilar particles should be so uniformly the same. If some of the particles of water were heavier than others, if a parcel of the liquid on any occasion were constituted principally of these heavier particles, it must be supposed to affect the specific gravity of the mass, a circumstance not known. Similar observations may be made on other substances. Therefore we may conclude that the ultimate particles of all homogeneous bodies are perfectly alike in weight, figure, etc. In other words, every particle of water is like every other particle of water; every particle of hydrogen is like every other particle of hydrogen, etc.

XXIII

MARIE FRANÇOIS XAVIER BICHAT

1771-1802

Bichat was born in the French town of Thoirette (Department of Ain), November 14, 1771. At the University of Lyons he was especially interested in anatomy, surgery, and natural history. In 1793, because of the Revolution, he fled to Paris, where he studied under the eminent surgeon Desault. In 1800 he distinguished between animal and organic functions and after many dissections he developed, in 1801, his famous doctrine of tissues. He died July 22, 1802, from injuries received in a fall.

THE DOCTRINE OF TISSUES *

OBJECT OF THE WORK

The general doctrine of this work has not precisely the character of any of those which have prevailed in medicine. Opposed to that of Boerhaave, it differs from that of Stahl and those authors who, like him, refer everything in the living economy to a single principle, purely speculative, ideal, and imaginary, whether designated by the name of soul, vital principle, or archeus. The general doctrine of this work consists in analyzing with precision the properties of living bodies, in showing that every physiological phenomenon is ultimately referable to these properties considered in their natural state; that every pathological phenomenon derives from them augmentation, diminution, or alteration; that every therapeutic phenomenon has for its principle the restoration of that part of the natural type, from which it has been changed; in determining with precision the cases in which each property is brought into action; in distinguishing accurately in physiology

* Translated from *Traité sur les Membranes* (1800).

as well as in medicine, that which is derived from one, and that which flows from others; in ascertaining by rigorous induction the natural and morbid phenomena which the animal properties produce, and those which are derived from the organic; and in pointing out when the animal sensibility and contractility are brought into action, and when the organic sensibility and the sensible or insensible contractility. We shall be easily convinced upon reflection, that we cannot precisely estimate the immense influence of the vital properties in the physiological sciences, before we have considered these properties in the point of view in which I have presented them. It will be said, perhaps, that this manner of viewing them is still a theory; I will answer that it is a theory like that which shows in the physical sciences, gravity, elasticity, affinity, etc., as the primitive principles of the facts observed in these sciences. The relation of these properties as causes to the phenomena as effects, is an axiom so well known in physics, chemistry, astronomy, etc., at the present day, that it is unnecessary to repeat it. If this work establishes an analogous axiom in the physiological sciences, its object will be attained.

OBSERVATIONS UPON THE ORGANIZATION OF ANIMALS

The properties, whose influence we have just analyzed, are not absolutely inherent in the particles of matter that are the seat of them. They disappear when these scattered particles have lost their organic arrangement. It is to this arrangement that they exclusively belong; let us treat of it here in a general way.

All animals are an assemblage of different organs, which, executing each a function, concur in their own manner, to the preservation of the whole. It is several separate machines in a general one, that constitutes the individual. Now these separate machines are themselves formed by many textures of a very different nature, and which really compose the elements of these organs. Chemistry has its simple bodies, which form, by the combination of which they are susceptible, the compound bodies; such are caloric, light, hydrogen, oxygen, carbon azote, phosphorus, etc. In the same way anatomy has its simple textures, which, by their combinations four with four, six with six, eight with eight, etc., make the organs. These textures, are, 1st, the cellular; 2d, the nervous of animal life; 3d, the nervous of organic

life; 4th, the arterial; 5th, the venous; 6th, the texture of the exhalants; 7th, that of the absorbents and their glands; 8th, the osseous; 9th, the medullary; 10th, the cartilaginous; 11th, the fibrous; 12th, the fibro-cartilaginous; 13th, the muscular of animal life; 14th, the muscular of organic life; 15th, the mucous; 16th, the serous; 17th, the synovial; 18th, the glandular; 19th, the dermoid; 20th, the epidermoid; 21st, the pilous.

These are the true organized elements of our bodies. Their nature is constantly the same, wherever they are met with. As in chemistry, the simple bodies do not alter, notwithstanding the different compound ones they form. The organized elements of man form the particular object of this work.

The idea of thus considering abstractly the different simple textures of our bodies, is not the work of the imagination; it rests upon the most substantial foundation, and I think it will have a powerful influence upon physiology as well as practical medicine. Under whatever point of view we examine them, it will be found that they do not resemble each other; it is nature and not science that has drawn the line of distinction between them.

1st. Their forms are everywhere different; here they are flat, there round. We see the simple textures arranged as membranes, canals, fibrous fasciæ, etc. No one has the same external character with another, considered as to their attributes of thickness or size. These differences of form, however, can only be accidental, and the same texture is sometimes seen under many different appearances; for example, the nervous appears as a membrane in the retina, and as cords in the nerves. This has nothing to do with their nature; it is then from the organization of the properties that the principal differences should be drawn.

2dly. There is no analogy in the organization of the simple textures. We shall see that this organization results from parts that are common to all, and from those that are peculiar to each; but the common parts are all differently arranged in each texture. Some unite in abundance the cellular texture, the blood vessels and the nerves; in others, one or two of these three common parts are scarcely evident or entirely wanting. Here there are only the exhalants and absorbents of nutrition; there the vessels are more numerous for other purposes. The capillary network, wonderfully multiplied, exists in certain tex-

tures; in others this network can hardly be demonstrated. As to the peculiar part, which essentially distinguishes the texture, the differences are striking. Color, thickness, hardness, density, resistance, etc., nothing is similar. More inspection is sufficient to show a number of characteristic attributes of each clearly different from the others. Here is a fibrous arrangement, there a granulated one; here it is lamellated, there circular. Notwithstanding these differences, authors are not agreed as to the limits of the different textures. I have had recourse, in order to leave no doubt upon this point, to the action of different re-agents. I have examined every texture, submitted them to the action of caloric, air, water, the acids, the alkalies, the neutral salts, etc., drying, putrefaction, maceration, boiling, etc.; the products of many of these actions have altered in a different manner each kind of texture. Now it will be seen that the results have almost all been different, that in these various changes each acts in a particular way, each gives results of its own, no one resembling another.

There has been considerable inquiry to ascertain whether the arterial coats are fleshy, whether the veins are of an analogous nature, etc. By comparing the results of my experiments upon the different textures, the question is easily resolved. It would seem at first view that all these experiments upon the intimate texture of systems answer but little purpose; I think, however, that they have effected a useful object, in fixing with precision the limits of each organized texture; for the nature of these textures being unknown, their differences can be ascertained only by the different results they furnish.

3rdly. In giving to each system a different organic arrangement, nature has also endowed them with different properties. You will see in the subsequent part of this work, that what we call texture presents degrees indefinitely varying, from the muscles, the skin, the cellular membrane, etc., which enjoy it in the highest degree, to the cartilages, the tendons, the bones, etc., which are almost destitute of it. Shall I speak of the vital properties? See the animal sensibly predominant in the nerves, contractility of the same kind particularly marked in the voluntary muscles, sensible organic contractility, forming the peculiar property of the involuntary, insensible contractility and sensibility of the same nature, which is not separated from it more than from the preceding, characterizing especially the glands, the skin, the serous

surfaces, etc., etc. See each of these simple textures combining, in different degrees, more or less of these properties, and consequently living with more or less energy.

There is but little difference arising from the number of vital properties they have in common; when these properties exist in many, they take in each a distinctive and peculiar character. This character is chronic, if I may so express myself, in the bones, the cartilages, the tendons, etc.; it is acute in the muscles, the skin, the glands, etc.

Independently of this general difference, each texture has a particular kind of force, of sensibility, etc. Upon this principle rests the whole theory of secretion, of exhalation, of absorption, and of nutrition. The blood is a common reservoir, from which each texture chooses that which is adapted to its sensibility, to appropriate and keep it, and afterwards reject it.

Much has been said since the time of Bordeu, of the peculiar life of each organ, which is nothing else than that particular character which distinguishes the combination of the vital properties of one organ from those of another. Before these properties had been analyzed with exactness and precision, it was clearly impossible to form a correct idea of this peculiar life. From the recount I have just given of it, it is evident that the greatest part of the organs being composed of very different simple textures, the idea of a peculiar life can only apply to these simple textures, and not to the organs themselves.

Some examples will render the point of doctrine which is important, more evident. The stomach is composed of the serous, organic muscular, mucous, and of almost all the common textures, as the arterial, the venous, etc., which we can consider separately. Now if you should attempt to describe in a general manner, the peculiar life of the stomach, it is evidently impossible that you could give a very precise and exact idea of it. In fact the mucous surface is so different from the serous, and both so different from the muscular, that by associating them together, the whole would be confused. The same is true of the intestines, the bladder, the womb, etc.; if you do not distinguish what belongs to each of the textures that form the compound organs, the term peculiar life will offer nothing but vagueness and uncertainty. This is so true, that oftentimes the same textures alternately belong or are foreign to their organs. The same portion of

the peritoneum, for example, enters or does not enter, into the gastric viscera, according to their fulness or vacuity.

Shall I speak of the pectoral organs? What has the life of the fleshy texture of the heart in common with that of the membrane that surrounds it? Is not the pleura independent of the pulmonary texture? Has this texture nothing in common with the membrane that surrounds the bronchia? Is it not the same with the brain with relation to its membranes, of the different parts of the eye, the ear, etc.?

When we study a function it is necessary carefully to consider in a general manner, the compound organ that performs it; but when you wish to know the properties and life of this organ, it is absolutely necessary to decompose it. In the same way, if you seek only general notions of anatomy, you can study each organ as a whole; but it is essential to separate the textures, if you have a desire to analyze with accuracy its intimate structure.

CONSEQUENCES OF THE PRECEDING PRINCIPLES RELATIVE TO DISEASE

What I have been saying leads to important consequences, as it respects those acute or chronic diseases that are local; for those which, like most fevers, affect almost simultaneously every part, cannot be much elucidated by the anatomy of systems. The first then will engage our attention.

Since diseases are only alterations of the vital properties, and each texture differs from the others in its properties, it is evident that there must be a difference also in the diseases. In every organ, then, composed of different textures, one may be diseased, while the others remain sound; now this happens in a great many cases; let us take the principal organs, for example.

1st. Nothing is more rare than affections of the mass of the brain; nothing is more common than inflammation of the *tunica arachnoides* that covers it. 2d. Oftentimes one membrane of the eye only is affected, the others preserving their ordinary degree of vitality. 3d. In convulsions or paralysis of the muscles of the larynx, the mucous surface is unaffected; and on the other hand, the muscles perform their functions as usual in catarrhs of this surface. Both these affections

are foreign to the cartilages, and *vice versa*. 4th. We observe a variety of different alterations in the texture of the pericardium, but hardly ever in that of the heart itself; it remains sound while the other is inflamed. The ossification of the common membrane of the red blood does not extend to the neighboring textures. 5th. When the membrane of the bronchia is the seat of catarrh, the pleura is hardly affected at all, and reciprocally in pleurisy the first is scarcely ever altered. In peripneumonia, when an enormous infiltration in the dead body shows the excessive inflammation that has existed during life in the pulmonary texture, the serous and mucous surfaces often appear not to have been affected. Those who open dead know that they are frequently healthy in incipient phthisis. 6th. We speak of a bad stomach, a weak stomach; this most commonly should be understood as applying to the mucous surface only. Whilst this secretes with difficulty the nutritive juices, without which digestion is impaired, the serous surface exhales as usual its fluid, the muscular coat continues to contract, etc. In ascites, in which the serous surface exhales more lymph than in a natural state, the mucous oftentimes performs its functions perfectly well, etc. 7th. All authors have said much of the inflammation of the stomach, the intestines, the bladder, etc. For myself, I believe that this disease rarely ever affects at first the whole of any of these organs, except in the case where poison or some other deleterious substance acts upon them. There are for the mucous surface of the stomach and intestines, acute and chronic catarrhs; for the peritoneum serous inflammations; perhaps even for the layer of organic muscles that separates the two membranes, there is a particular kind of inflammation, though we have as yet hardly anything certain upon this point; but the stomach, the intestines, and the bladder are not suddenly affected with these three diseases. A diseased texture can affect those near it, but the primitive affection seizes only upon one. I have examined a great number of bodies in which the peritoneum was inflamed either upon the intestines, the stomach, the pelvis, or universally; now very often when this affection is chronic, and almost always when it is acute, the subjacent organs remain sound. I have never seen this membrane exclusively diseased upon one organ, while that of neighboring ones remain untouched; its affection is propagated more or less remotely. I know not why authors have hardly ever spoken of its in-

flammation, and have placed to the account of the subjacent viscera that which most often belongs only to this. There are almost as many cases of peritonitis as of pleurisy, and yet while these last have been particularly noticed the others are almost entirely overlooked. Oftentimes that part of the peritoneum corresponding to an organ, is much inflamed; we see it in the case of the stomach; we observe especially after the suppression of the lochia or the menses, that it is the portion that lines the pelvis that is first affected. But soon the affection becomes more or less general; at least examinations after death prove it satisfactorily. 8th. Certainly the acute or chronic catarrh of the bladder, or womb even, has nothing in common with the inflammation of that portion of the peritoneum corresponding with these organs. 9th. Every one knows that diseases of the periosteum have oftentimes no connection with the bone, and *vice versa*, that frequently the marrow is for a long time affected, while both the others remain sound. There is no doubt that the osseous, medullary and fibrous textures have their peculiar affections which we shall not confound with the idea we may form of the diseases of the bones. The same can be said of the intestines, of the stomach, etc., in relation to their mucous, serous, muscular textures, etc. 10th. Though the muscular and tendinous textures are combined in a muscle, their diseases are very different. 11th. You must not think that the synovial is subject to the same diseases as the ligaments that surround it, etc.

I think the more we observe diseases, and the more we examine bodies, the more we shall be convinced of the necessity of considering local diseases, not under the relations of the compound organs, which are rarely ever affected as a whole, but under that of their different textures, which are almost always attacked separately.

When the phenomena of disease are sympathetic, they follow the same laws as when they arise from a direct affection. Much has been said of the sympathies of the stomach, the intestines, the bladder, the lungs, etc. But it is impossible to form an idea of them, if they are referred to the organ as a whole, separate from the different textures. 1st. When in the stomach, the fleshy fibres contract by the influence of another organ and produce vomiting, they alone receive the influence, which is not extended either to the serous or mucous surfaces; if it were, they would be the seat, the one of exhalation, the other of sympathetic exhalation and secretion. 2d. It

is certain that when the action of the liver is sympathetically increased, so that it pours out more bile, the portion of peritoneum that covers it does not throw out more serum, because it is not affected by it. It is the same of the kidney, the pancreas, etc. 3d. For the same reason the gastric organs upon which the peritoneum is spread do not partake of the sympathetic influences that it experiences. I shall say as much of the lungs in relation to the pleura, the brain in relation to the *tunica arachnoides*, the heart to the pericardium, etc. 4th. It is undeniable that in all sympathetic convulsions, the fleshy texture alone is affected, and that the tendinous is not so at all. 5th. What has the fibrous membrane of the testicles in common with the sympathies of its peculiar texture? 6th. No doubt a number of sympathetic pains that we refer to the bones, are seated exclusively in the marrow.

I could cite many other examples to prove, that it is not this or that organ that sympathizes as a whole, but only this or that texture in the organs; besides, this an immediate consequence of the nature of sympathies. In fact the sympathies are but aberrations of the vital properties; now these properties vary according to each texture; the sympathies of these textures then would do the same.

XXIV

AMADEO AVOGADRO

1776-1856

Avogadro, who continued the researches of Dalton and Gay-Lussac, was born in Turin, Italy, June 9, 1776. In 1796, after receiving the doctor's degree in law from the University of Turin, he was employed by the government for the following ten years. He began his work in science in 1806 and three years later was made professor of physics at Vercelli. In 1811 he announced his famous law. According to Merz, since the time of Boyle "it had been known that equal volumes of different gases under equal pressure change their volumes equally if the pressure is varied equally, and it was also known that equal volumes of different gases under equal pressure change their volumes equally with equal rise of temperature. These facts suggested to Avogadro, and almost simultaneously to Ampère, the very simple assumption that this is owing to the fact that equal volumes of different gases contain an equal number of the smallest independent particles of matter. This is Avogadro's celebrated hypothesis. It was the first step in the direct physical verification of the atomic view of matter."

In 1820 Avogadro became professor of physics at Turin University, where he remained for many years. He died July 9, 1856.

THE MOLECULES IN GASES PROPORTIONAL TO THE
VOLUMES *

I.

M. Gay-Lussac has shown in an interesting Memoir (*Mémoires de la Société d'Arcueil*, Tome II.) that gases always unite in a very

* Translated from *Essai d'une manière de déterminer les masses relatives des molécules élémentaires des corps, et les proportions selon lesquelles elles entrent dans les combinaisons*—*Journal de Physique*, (1811).

simple proportion by volume, and that when the result of the union is a gas, its volume also is very simply related to those of its components. But the quantitative proportions of substances in compounds seem only to depend on the relative number of molecules which combine, and on the number of composite molecules which result. It must then be admitted that very simple relations also exist between the volumes of gaseous substances and the numbers of simple or compound molecules which form them. The first hypothesis to present itself in this connection, and apparently even the only admissible one, is the supposition that the number of integral molecules in any gases is always the same for equal volumes, or always proportional to the volumes. Indeed, if we were to suppose that the number of molecules contained in a given volume were different for different gases, it would scarcely be possible to conceive that the law regulating the distance of molecules could give in all cases relations so simple as those which the facts just detailed compel us to acknowledge between the volume and the number of molecules. On the other hand, it is very well conceivable that the molecules of gases being at such a distance that their mutual attraction cannot be exercised, their varying attraction for caloric may be limited to condensing a greater or smaller quantity around them, without the atmosphere formed by this fluid having any greater extent in the one case than in the other, and, consequently, without the distance between the molecules varying; or, in other words, without the number of molecules contained in a given volume being different. Dalton, it is true, has proposed a hypothesis directly opposed to this, namely, that the quantity of caloric is always the same for the molecules of all bodies whatsoever in the gaseous state, and that the greater or less attraction for caloric only results in producing a greater or less condensation of this quantity around the molecules, and thus varying the distance between the molecules themselves. But in our present ignorance of the manner in which this attraction of the molecules for caloric is exerted, there is nothing to decide us *a priori* in favour of the one of these hypotheses rather than the other; and we should rather be inclined to adopt a neutral hypothesis, which would make the distance between the molecules and the quantities of caloric vary according to unknown laws, were it not that the hypothesis we have just proposed is based on that simplicity of relation between the volumes of gases

on combination, which would appear to be otherwise inexplicable.

Setting out from this hypothesis, it is apparent that we have the means of determining very easily the relative masses of the molecules of substances obtainable in the gaseous state, and the relative number of these molecules in compounds; for the ratios of the masses of the molecules are then the same as those of the densities of the different gases at equal temperature and pressure, and the relative number of molecules in a compound is given at once by the ratio of the volumes of the gases that form it. For example, since the numbers 1.10359 and 0.07321 express the densities of the two gases oxygen and hydrogen compared to that of atmospheric air as unity, and the ratio of the two numbers consequently represents the ratio between the masses of equal volumes of these two gases, it will also represent on our hypothesis the ratio of the masses of their molecules. Thus the mass of the molecule of oxygen will be about 15 times that of the molecule of hydrogen, or, more exactly, as 15.074 to 1. In the same way the mass of the molecule of nitrogen will be to that of hydrogen as 0.96913 to 0.07321, that is, as 13, or more exactly 13.238, to 1. On the other hand, since we know that the ratio of the volumes of hydrogen and oxygen in the formation of water is 2 to 1, it follows that water results from the union of each molecule of oxygen with two molecules of hydrogen. Similarly, according to the proportions by volume established by M. Gay-Lussac for the elements of ammonia, nitrous oxide, nitrous gas, and nitric acid, ammonia will result from the union of one molecule of nitrogen with three of hydrogen, nitrous oxide from one molecule of oxygen with two of nitrogen, nitrous gas from one molecule of nitrogen with one of oxygen, and nitric acid from one of nitrogen with two of oxygen.

II.

There is a consideration which appears at first sight to be opposed to the admission of our hypothesis with respect to compound substances. It seems that a molecule composed of two or more elementary molecules should have its mass equal to the sum of the masses of these molecules; and that in particular, if in a compound one molecule of one substance unites with two or more molecules of another substance, the number of compound molecules should remain the same

as the number of molecules of the first substance. Accordingly, on our hypothesis when a gas combines with two or more times its volume of another gas, the resulting compound, if gaseous, must have a volume equal to that of the first of these gases. Now, in general, this is not actually the case. For instance, the volume of water in the gaseous state is, as M. Gay-Lussac has shown, twice as great as the volume of oxygen which enters into it, or, what comes to the same thing, equal to that of the hydrogen instead of being equal to that of the oxygen. But a means of explaining facts of this type in conformity with our hypothesis presents itself naturally enough: we suppose, namely, that the constituent molecules of any simple gas whatever (i. e., the molecules which are at such a distance from each other that they cannot exercise their mutual action) are not formed of a solitary elementary molecule, but are made up of a certain number of these molecules united by attraction to form a single one; and further, that when molecules of another substance unite with the former to form a compound molecule, the integral molecule which should result splits up into two or more parts (or integral molecules) composed of half, quarter, &c., the number of elementary molecules going to form the constituent molecule of the first substance, combined with half, quarter, &c., the number of constituent molecules of the second substance that ought to enter into combination with one constituent molecule of the first substance (or, what comes to the same thing, combined with a number equal to this last of half-molecules, quarter-molecules, &c., of the second substance); so that the number of integral molecules of the compound becomes double, quadruple, &c., what it would have been if there had been no splitting-up, and exactly what is necessary to satisfy the volume of the resulting gas.

On reviewing the various compound gases most generally known, I only find examples of duplication of the volume relatively to the volume of that one of the constituents which combines with one or more volumes of the other. We have already seen this for water. In the same way, we know that the volume of ammonia gas is twice that of the nitrogen which enters into it. M. Gay-Lussac has also shown that the volume of nitrous oxide is equal to that of the nitrogen which forms part of it, and consequently is twice that of the oxygen. Finally, nitrous gas, which contains equal volumes of nitrogen and

oxygen, has a volume equal to the sum of the two constituent gases, that is to say, double that of each of them. Thus in all these cases there must be a division of the molecule into two; but it is possible that in other cases the division might be into four, eight, &c. The possibility of this division of compound molecules might have been conjectured *a priori*; for otherwise the integral molecules of bodies composed of several substances with a relatively large number of molecules, would come to have a mass excessive in comparison with the molecules of simple substances. We might therefore imagine that nature had some means of bringing them back to the order of the latter, and the facts have pointed out to us the existence of such means. Besides, there is another consideration which would seem to make us admit in some cases the division in question; for how could one otherwise conceive a real combination between two gaseous substances uniting in equal volumes without condensation, such as takes place in the formation of nitrous gas? Supposing the molecules to remain at such a distance that the mutual attraction of those of each gas could not be exercised, we cannot imagine that a new attraction could take place between the molecules of one gas and those of the other. But on the hypothesis of division of the molecule, it is easy to see that the combination really reduces two different molecules to one, and that there would be contraction by the whole volume of one of the gases if each compound molecule did not split up into two molecules of the same nature. M. Gay-Lussac clearly saw that, according to the facts, the diminution of volume on the combination of gases cannot represent the approximation of their elementary molecules. The division of molecules on combination explains to us how these two things may be made independent of each other.

III.

Dalton, on arbitrary suppositions as to the most likely relative number of molecules in compounds, has endeavoured to fix ratios between the masses of the molecules of simple substances. Our hypothesis, supposing it well founded, puts us in a position to confirm or rectify his results from precise data, and, above all, to assign the magnitude of compound molecules according to the volumes of the gaseous compounds, which depend partly on the division of molecules entirely unsuspected by this physicist.

Thus Dalton supposes that water is formed by the union of hydrogen and oxygen, molecule to molecule. From this, and from the ratio by weight of the two components, it would follow that the mass of the molecule of oxygen would be to that of hydrogen as $7\frac{1}{2}$ to 1 nearly, or, according to Dalton's evaluation, as 6 to 1. This ratio on our hypothesis is, as we saw, twice as great, namely, as 15 to 1. As for the molecule of water, its mass ought to be roughly expressed by $15+2=17$ (taking for unity that of hydrogen), if there were no division of the molecule into two; but on account of this division it is reduced to half, $8\frac{1}{2}$, or more exactly 8.537, as may also be found directly by dividing the density of aqueous vapour 0.625 (Gay-Lussac) by the density of hydrogen 0.0732. This mass only differs from 7, that assigned to it by Dalton, by the difference in the values for the composition of water; so that in this respect Dalton's result is approximately correct from the combination of two compensating errors,—the error in the mass of the molecule of oxygen, and his neglect of the division of the molecule.

XXV

SIR HUMPHREY DAVY

1778-1829

Born December 17, 1778, in Cornwall, Sir Humphrey Davy was apprenticed in 1794 to a surgeon-apothecary at Penzance in whose service he became interested in chemistry. Made superintendent of a hospital in 1798, he had opportunities for gaining acquaintance with influential men who in turn recommended him to Count Rumford. Through the latter's assistance he was appointed lecturer on chemistry at the newly-founded Royal Institution where, in spite of his unattractive appearance, he gained considerable reputation. In 1807 he advanced a theory which partly explained electrolysis; in the following year he discovered strontium and magnesium; and in 1809, chlorine. In 1812 he was knighted; and shortly after his marriage, in the same year, he injured an eye while experimenting and was compelled to interrupt his work for a short time. In 1815 he invented the safety-lamp used by miners. In 1818 he was created a baronet, and was elected President of the Royal Society in 1820. He died May 29, 1829, at Geneva, Switzerland, at the age of fifty-one.

ON SOME NEW PHENOMENA OF CHEMICAL CHANGES PRODUCED BY ELECTRICITY *

Read November 19, 1807.

INTRODUCTION.

In the Bakerian Lecture which I had the honour of presenting to the Royal Society last year, I described a number of decompositions

* From the *Transactions of the Royal Society of London.*

and chemical changes produced in substances of known composition by electricity, and I ventured to conclude from the general principles on which the phenomena were capable of being explained, that the new methods of investigation promised to lead to a more intimate knowledge than had hitherto been obtained, concerning the true elements of bodies.

This conjecture, then sanctioned only by strong analogies, I am now happy to be able to support by some conclusive facts. In the course of a laborious experimental application of the powers of electrochemical analysis, to bodies which have appeared simple when examined by common chemical agents, or which at least have never been decomposed, it has been my good fortune to obtain new and singular results.

Such of the series of experiments as are in a tolerably mature state, and capable of being arranged in a connected order, I shall detail in the following sections, particularly those which demonstrate the decomposition and composition of the fixed alkalies, and the production of the new and extraordinary bodies which constitute their bases.

In speaking of novel methods of investigation, I shall not fear to be minute. When the common means of chemical research have been employed, I shall mention only results. A historical detail of the progress of the investigation, of all the difficulties that occurred, and of the manner in which they were overcome, and of all the manipulations employed, would far exceed the limits assigned to this Lecture. It is proper to state, however, that when general facts are mentioned, they are such only as have been deduced from processes carefully performed and often repeated.

ON THE METHODS USED FOR THE DECOMPOSITION OF THE FIXED ALKALIES

The researches I had made on the decomposition of acids, and of alkaline and earthy neutral compounds, proved that the powers of electrical decomposition were proportional to the strength of the opposite electricities in the circuit, and to the conducting power and degree of concentration of the materials employed.

In the first attempts, that I made on the decomposition of the fixed alkalies, I acted upon aqueous solutions of potash and soda, satu-

rated at common temperatures, by the highest electrical power I could command, and which was produced by a combination of Voltaic batteries belonging to the Royal Institution, containing 24 plates of copper and zinc of 12 inches square, 100 plates of 6 inches, and 150 of 4 inches square, charged with solutions of alum and nitrous acid; but in these cases, though there was a high intensity of action, the water of the solutions alone was affected, and hydrogen and oxygen disengaged with the production of much heat and violent effervescence.

The presence of water appearing thus to prevent any decomposition, I used potash in igneous fusion. By means of a stream of oxygen gas from a gasometer applied to the flame of a spirit lamp, which was thrown on a platina spoon containing potash, this alkali was kept for some minutes in a strong red heat, and in a state of perfect fluidity. The spoon was preserved in communication with the positive side of the battery of the power of 100 of 6 inches, highly charged; and the connection from the negative side was made by a platina wire.

By this arrangement some brilliant phenomena were produced. The potash appeared a conductor in a high degree, and as long as the communication was preserved, a most intense light was exhibited at the negative wire, and a column of flame, which seemed to be owing to the development of combustible matter, arose from the point of contact.

When the order was changed, so that the platina spoon was made negative, a vivid and constant light appeared at the opposite point: there was no effect of inflammation round it; but aeriform globules, which inflamed in the atmosphere, rose through the potash.

The platina, as might have been expected, was considerably acted upon; and in the cases when it had been negative, in the highest degree.

The alkali was apparently dry in this experiment; and it seemed probable that the inflammable matter arose from its decomposition. The residual potash was unaltered; it contained indeed a number of dark grey metallic particles, but these proved to be derived from the platina.

I tried several experiments on the electrization of potash rendered fluid by heat, with the hopes of being able to collect the combustible

matter, but without success; and I only attained my object by employing electricity as the common agent for fusion and decomposition.

Though potash, perfectly dried by ignition, is a non-conductor, yet it is rendered a conductor by a very slight addition of moisture, which does not perceptibly destroy its aggregation; and in this state it readily fuses and decomposes by strong electrical powers.

A small piece of pure potash, which had been exposed for a few seconds to the atmosphere, so as to give conducting power to the surface, was placed upon an insulated disc of platina, connected with the negative side of the battery of the power of 250 of 6 and 4, in a state of intense activity; and a platina wire, communicating with the positive side, was brought in contact with the upper surface of the alkali. The whole apparatus was in the open atmosphere.

Under these circumstances a vivid action was soon observed to take place. The potash began to fuse at both its points of electrization. There was a violent effervescence at the upper surface; at the lower, or negative surface, there was no liberation of elastic fluid; but small globules having a high metallic lustre, and being precisely similar in visible characters to quicksilver, appeared, some of which burnt with explosion and bright flame, as soon as they were formed, and others remained, and were merely tarnished, and finally covered by a white film which formed on their surfaces.

These globules, numerous experiments soon showed to be the substance I was in search of, and a peculiar inflammable principle the basis of potash. I found that the platina was in no way connected with the result, except as the medium for exhibiting the electrical powers of decomposition; and a substance of the same kind was produced when pieces of copper, silver, gold, plumbago, or even charcoal were employed for completing the circuit.

The phenomenon was independent of the presence of air; I found that it took place when the alkali was in the vacuum of an exhausted receiver.

The substance was likewise produced from potash fused by means of a lamp, in glass tubes confined by mercury, and furnished with hermetically inserted platina wires by which the electrical action was transmitted. But this operation could not be carried on for any considerable time; the glass was rapidly dissolved by the action of

the alkali, and this substance soon penetrated through the body of the tube.

Soda, when acted upon in the same manner as potash, exhibited an analogous result; but the decomposition demanded greater intensity of action in the batteries, or the alkali was required to be in much thinner and smaller pieces. With the battery of 100 of 6 inches in full activity I obtained good results from pieces of potash weighing from 40 to 70 grains, and of a thickness which made the distance of the electrified metallic surfaces nearly a quarter of an inch; but with a similar power it was impossible to produce the effects of decomposition on pieces of soda of more than 15 or 20 grains in weight, and that only when the distance between the wires was about 1·8 or 1·10 of an inch.

The substance produced from potash remained fluid at the temperature of the atmosphere at the time of its production; that from soda, which was fluid in the degree of heat of the alkali during its formation, became solid on cooling, and appeared having the lustre of silver.

When the power of 250 was used, with a very high charge for the decomposition of soda, the globules often burnt at the moment of their formation, and sometimes violently exploded and separated into smaller globules, which flew with great velocity through the air in a state of vivid combustion, producing a beautiful effect of continued jets of fire.

THEORY OF THE DECOMPOSITION OF THE FIXED ALKALIES; THEIR COMPOSITION AND PRODUCTION

As in all decompositions of compound substances which I had previously examined, at the same time that combustible bases were developed at the negative surface in the electrical circuit, oxygen was produced, and evolved or carried into combination at the positive surface, it was reasonable to conclude that this substance was generated in a similar manner by the electrical action upon the alkalies; and a number of experiments made above mercury, with the apparatus for excluding external air, proved that this was the case.

When solid potash, or soda in its conducting state, was included

in glass tubes furnished with electrified platina wires, the new substances were generated at the negative surfaces; the gas given out at the other surface proved by the most delicate examination to be pure oxygen; and unless an excess of water was present, no gas was evolved from the negative surface.

In the synthetical experiments, a perfect coincidence likewise will be found.

I mentioned that the metallic lustre of the substance from potash immediately became destroyed in the atmosphere, and that a white crust formed upon it. This crust I soon found to be pure potash, which immediately deliquesced, and new quantities were formed, which in their turn attracted moisture from the atmosphere till the whole globule disappeared, and assumed the form of a saturated solution of potash.

When globules were placed in appropriate tubes containing common air or oxygen gas confined by mercury, an absorption of oxygen took place; a crust of alkali instantly formed upon the globule; but from the want of moisture for its solution, the process stopped, the interior being defended from the action of the gas.

With the substance from soda, the appearances and effects were analogous.

When the substances were strongly heated, confined in given proportions of oxygen, a rapid combustion with a brilliant white flame was produced, and the metallic globules were found converted into a white and solid mass, which in the case of the substance from potash was found to be potash, and in the case of that from soda, soda.

Oxygen gas was absorbed in this operation, and nothing emitted which affected the purity of the residual air.

The alkalies produced were apparently dry, or at least contained no more moisture than might well be conceived to exist in the oxygen gas absorbed; and their weights considerably exceeded those of the combustible matters consumed.

The processes on which these conclusions are founded will be fully described hereafter, when the minute details which are necessary will be explained, and the proportions of oxygen, and of the respective inflammable substances which enter into union to form the fixed alkalies, will be given.

It appears, then, that in these facts there is the same evidence for

the decomposition of potash and soda into oxygen and two peculiar substances, as there is for the decomposition of sulphuric and phosphoric acids and the metallic oxides into oxygen and their respective combustible bases.

In the analytical experiments, no substances capable of decomposition are present but the alkalis and a minute portion of moisture; which seems in no other way essential to the result, than in rendering them conductors at the surface: for the new substances are not generated till the interior, which is dry, begins to be fused; they explode when in rising through the fused alkali they come in contact with the heated moistened surface; they cannot be produced from crystallised alkalis, which contain much water; and the effect produced by the electrization of ignited potash, which contains no sensible quantity of water, confirms the opinion of their formation independently of the presence of this substance.

The combustible bases of the fixed alkalis seem to be repelled as other combustible substances, by positively electrified surfaces, and attracted by negatively electrified surfaces, and the oxygen follows the contrary order; or the oxygen being naturally possessed of the negative energy, and the bases of the positive, do not remain in combination when either of them is brought into an electrical state opposite to its natural one. In the synthesis, on the contrary, the natural energies or attractions come in equilibrium with each other; and when these are in a low state at common temperatures, a slow combination is effected; but when they are exalted by heat, a rapid motion is the result; and as in other like cases with the production of fire.



XXVI

MICHAEL FARADAY,

1791-1867

Born on September 22, 1791, at Newington, Surrey, England, Michael Faraday was the son of a blacksmith. After an early and very elementary education, he was apprenticed in 1805 to a book-binder in whose service he read widely and thus educated himself. Developing an interest in physics, he attended the evening lectures of Sir Humphrey Davy who, in 1813, engaged him as an assistant. Seven years later he wrote a history of electro-magnetism and succeeded, in the same year, in getting a needle to rotate fully around a live wire. In 1823 he liquefied chlorine, an experiment which destroyed the old notion of the permanent distinction between gases and liquids. In 1831 he discovered magneto-electric induction and advanced the conception of "lines of magnetic force." In 1845, in trying to send polarized rays of light through heavy magnetized glass, he found that the magnet's action interrupted the passage of the light and that magnetization caused the plane of polarization to rotate. He died August 25, 1867.

ON FLUID CHLORINE *

Read March 13, 1823.

It is well known that before the year 1810, the solid substance obtained by exposing chlorine, as usually procured, to a low temperature, was considered as the gas itself reduced into that form; and that Sir Humphrey Davy first showed it to be a hydrate, the pure dry gas not being considerable even at a temperature of 40° F.

I took advantage of the late cold weather to procure crystals of this

* This excerpt and the one following are from the *Transactions of the Royal Society of London*.

substance for the purpose of analysis. The results are contained in a short paper in the *Quarterly Journal of Science*, Vol. XV. Its composition is very nearly 27.7 chlorine, 72.3 water, or 1 proportional of chlorine, and 10 of water.

The President of the Royal Society having honoured me by looking at these conclusions, suggested, that an exposure of the substance to heat under pressure, would probably lead to interesting results; the following experiments were commenced at his request. Some hydrate of chlorine was prepared, and being dried as well as could be by pressure in bibulous paper, was introduced into a sealed glass tube, the upper end of which was then hermetically closed. Being placed in water at 60° , it underwent no change; but when put into water at 100° , the substance fused, the tube became filled with a bright yellow atmosphere, and, on examination, was found to contain two fluid substances: the one, about three-fourths of the whole, was of a faint yellow colour, having very much the appearance of water; the remaining fourth was a heavy bright yellow fluid, lying at the bottom of the former, without any apparent tendency to mix with it. As the tube cooled, the yellow atmosphere condensed into more of the yellow fluid, which floated in a film on the pale fluid, looking very like chloride of nitrogen; and at 70° the pale portion congealed, although even at 32° the yellow portion did not solidify. Heated up to 100° the yellow fluid appeared to boil, and again produced the bright coloured atmosphere.

By putting the hydrate into a bent tube, afterwards hermetically sealed, I found it easy, after decomposing it by a heat of 100° , to distil the yellow fluid to one end of the tube, and so separate it from the remaining portion. In this way a more complete decomposition of the hydrate was effected, and, when the whole was allowed to cool, neither of the fluids solidified at temperatures above 34° , and the yellow portion not even at 0° . When the two were mixed together they gradually combined at temperatures below 60° , and formed the same solid substance as that first introduced. If, when the fluids were separated, the tube was cut in the middle, the parts flew asunder as if with an explosion, the whole of the yellow portion disappeared, and there was a powerful atmosphere of chlorine produced; the pale portion on the contrary remained, and when examined, proved to be a weak solution of chlorine in water, with a little muriatic acid, probably from the

impurity of the hydrate used. When that end of the tube in which the yellow fluid lay was broken under a jar of water, there was an immediate production of chlorine gas.

I at first thought that muriatic acid and euchlorine had been formed; then, that two new hydrates of chlorine had been produced; but at last I suspected that the chlorine had been entirely separated from the water by the heat and condensed into a dry fluid by the mere pressure of its own abundant vapour. If that were true, it followed, that chlorine gas, when compressed, should be condensed into the same fluid, and, as the atmosphere in the tube in which the fluid lay was not very yellow at 50° or 60° , it seemed probable that the pressure required was not beyond what could readily be obtained by a condensing syringe. A long tube was therefore furnished with a cap and stop-cock, then exhausted of air and filled with chlorine, and being held vertically with the syringe upwards, air was forced in, which thrust the chlorine to the bottom of the tube, and gave a pressure of about 4 atmospheres. Being now cooled, there was an immediate deposit in films, which appeared to be hydrate, formed by water contained in the gas and vessels, but some of the yellow fluid was also produced. As this however might also contain a portion of the water present, a perfectly dry tub and apparatus were taken, and the chlorine left for some time over a bath of sulphuric acid before it was introduced. Upon throwing in air and giving pressure, there was now no solid film formed, but the clear yellow fluid was deposited, and more abundantly still upon cooling. After remaining some time it disappeared, having gradually mixed with the atmosphere above it, but every repetition of the experiment produced the same results.

Presuming that I had now a right to consider the yellow fluid as pure chlorine in the liquid state, I proceeded to examine its properties, as well as I could when obtained by heat from the hydrate. However obtained, it always appears very limpid and fluid, and excessively volatile at common pressure. A portion was cooled in its tube to 0° ; it remained fluid. The tube was then opened, when a part immediately flew off, leaving the rest so cooled by the evaporation as to remain a fluid under the atmospheric pressure. The temperature could not have been higher than 40° in this case; as Sir Humphrey Davy has shown that dry chlorine does not condense at that temperature under common pressure. Another tube was opened at a tem-

perature of 50° ; a part of the chlorine volatilised, and cooled the tube so much as to condense the atmospheric vapour on it as ice.

A tube having the water at one end and the chlorine at the other was weighed, and then cut in two; the chlorine immediately flew off, and the loss being ascertained was found to be 1.6 grains: the water left was examined and found to contain some chlorine: its weight was ascertained to be 5.4 grains. These proportions, however, must not be considered as indicative of the true composition of hydrate of chlorine; for, from the mildness of the weather during the time when these experiments were made, it was impossible to collect the crystals of hydrate, press, and transfer them, without losing much chlorine; and it is also impossible to separate the chlorine and water in the tube perfectly, or keep them separate, as the atmosphere within will combine with the water, and gradually reform the hydrate.

Before cutting the tube, another tube had been prepared exactly like it in form and size, and a portion of water introduced into it, as near as the eye could judge, of the same bulk as the fluid chlorine: this water was found to weigh 1.2 grains; a result, which, if it may be trusted, would give the specific gravity of fluid chlorine as 1.33; and from its appearance in, and on water, this cannot be far wrong.

ELECTRICITY FROM MAGNETISM

Read November 24, 1831.

1. The power which electricity of tension possesses of causing an opposite electrical state in its vicinity has been expressed by the general term Induction; which, as it has been received into scientific language, may also, with propriety, be used in the same general sense to express the power which electrical currents may possess of inducing any particular state upon matter in their immediate neighborhood, otherwise indifferent. It is with this meaning that I purpose using it in the present paper.

2. Certain effects of the induction of electrical currents have already been recognized and described: as those of magnetization; Ampère's experiments of bringing a copper disc near to a flat spiral; his repetition with electro-magnets of Arago's extraordinary experiments, and perhaps a few others. Still it appeared unlikely that these

could be all the effects which induction by currents could produce; especially as, upon dispensing with iron, almost the whole of them disappear, whilst yet an infinity of bodies, exhibiting definite phenomena of induction with electricity of tension still remain to be acted upon by the induction of electricity in motion.

3. Further: whether Ampère's beautiful theory were adopted, or any other, or whatever reservation were mentally made, still it appeared very extraordinary, that, as every electric current was accompanied by a corresponding intensity of magnetic action at right angles to the current, good conductors of electricity, when placed within the sphere of this action, should not have any current induced through them, or some sensible effect produced equivalent in force to such a current.

4. These considerations, with their consequence, the hope of obtaining electricity from ordinary magnetism, have stimulated me at various times to investigate experimentally the inductive effect of electric currents. I lately arrived at positive results; and not only had my hopes fulfilled, but obtained a key which appeared to me to open out a full explanation of Arago's magnetic phenomena, and also to discover a new state, which may probably have great influence in some of the most important effects of electric currents.

5. These results I purpose describing, not as they were obtained, but in such a manner as to give the most concise view of the whole.

EVOLUTION OF ELECTRICITY FROM MAGNETISM

27. A welded ring was made of soft round bar-iron, the metal being seven-eighths of an inch in thickness, and the ring six inches in external diameter. Three helices were put round one part of this ring, each containing about twenty-four feet of copper wire one-twentieth of an inch thick; they were insulated from the iron and each other, and superposed in the manner before described (6), occupying about nine inches in length upon the ring. They could be used separately or conjointly; the group may be distinguished by the letter A. On the other part of the ring about sixty feet of similar copper wire in two pieces were applied in the same manner, forming a helix B, which had the same common direction with the helices of A, but being separated from it at each extremity by about half an inch of the uncovered iron.

28. The helix B, was connected by copper wires with a galvanometer three feet from the ring. The helices of A were connected end to end so as to form one common helix, the extremities of which were connected with a battery of ten pairs of plates four inches square. The galvanometer was immediately affected, and to a degree far beyond what has been described when with a battery of tenfold power helices without iron were used (10); but though the contact was continued, the effect was not permanent, for the needle soon came to rest in its natural position, as if quite indifferent to the attached electromagnetic arrangement. Upon breaking the contact with the battery, the needle was again powerfully deflected, but in the contrary direction to that induced in the first instance.

29. Upon arranging the apparatus so that B should be out of use, the galvanometer be connected with one of the three wires of A (27), and the other two made into a helix through which the current from the trough (28) was passed, similar but rather more powerful effects were produced.

30. When the battery contact was made in one direction, the galvanometer-needle was deflected on the one side; if made in the other direction, the deflection was on the other side. The deflection on breaking the battery contact was always the reverse of that produced by completing it. The deflection on making a battery contact always indicated an induced current in the opposite direction to that from the battery; but on breaking the contact the deflection indicated an induced current in the same direction as that of the battery. No making or breaking of the contact at B side, or in any part of the galvanometer circuit, produced any effect at the galvanometer. No continuance of the battery current caused any deflection of the galvanometer-needle. As the above results are common to all these experiments, and to similar ones with ordinary magnets to be hereafter detailed, they need not be again particularly described.

31. Upon using the power of 100 pairs of plates (10) with this ring, the impulse at the galvanometer, when contact was completed or broken, was so great as to make the needle spin round rapidly four or five times, before the air and terrestrial magnetism could reduce its motion to mere oscillation.

39. But as might be supposed that in all the preceding experiments of this section, it was by some peculiar effect taking place during the

formation of the magnet, and not by its mere virtual approximation, that the momentary induced current was excited, the following experiment was made. All the similar ends of the compound hollow helix (34) were bound together by copper wire, forming two general terminations, and these were connected with the galvanometer. The soft iron cylinder (34) was removed, and a cylindrical magnet three-quarters of an inch in diameter and eight inches and a half in length, used instead. One end of this magnet was introduced into the axis of the helix and then, the galvanometer-needle being stationary, the magnet was suddenly thrust in; immediately the needle was deflected in the same direction as if the magnet had been formed by either of the two preceding processes (34, 36). Being left in, the needle resumed its first position, and then the magnet being withdrawn the needle was deflected in the opposite direction. These effects were not great; but by introducing and withdrawing the magnet, so that the impulse each time should be added to those previously communicated to the needle, the latter could be made to vibrate through an arc of 180° or more.

40. In this experiment the magnet must not be passed entirely through the helix, for then a second action occurs. When the magnet is introduced the needle at the galvanometer is deflected in a certain direction; but being in, whether it be pushed quite through or withdrawn, the needle is deflected in a direction the reverse of that previously produced. When the magnet is passed in and through at one continuous motion, the needle moves one way, is then suddenly stopped, and finally moves the other way.

41. If such a hollow helix as that described (34) be laid east and west (or in any other constant position), and a magnet be retained east and west, its marked pole always being one way; then whichever end of the helix the magnet goes in at, and consequently whichever pole of the magnet enters first, still the needle is deflected the same way: on the other hand, whichever direction is followed in withdrawing the magnet, the deflection is constant, but contrary to that due to its entrance.

57. The various experiments of this section prove, I think, most completely the production of electricity from ordinary magnetism. That its intensity should be very feeble and quantity small, cannot be considered wonderful, when it is remembered that like thermo-

electricity it is evolved entirely within the substance of metals retaining all their conducting power. But an agent which is conducted along the metallic wires in the manner described; which, whilst so passing possesses the peculiar magnetic actions and force of a current of electricity; which can agitate and convulse the limbs of a frog; and which, finally, can produce a spark by its discharge through charcoal (32), can only be electricity. As all the effects can be produced by ferruginous electro-magnets (34), there is no doubt that arrangements like the magnets of Professors Moll, Henry, Ten Eyke, and others, in which as many as two thousand pounds have been lifted, may be used for these experiments; in which case not only a brighter spark may be obtained, but wires also ignited, and, as the current can pass liquids (23), chemical action be produced. These effects are still more likely to be obtained when the magneto-electric arrangements to be explained in the fourth section are excited by the powers of such apparatus.

58. The similarity of action, almost amounting to identity, between common magnets and either electro-magnets or volta-electric currents, is strikingly in accordance with and confirmatory of M. Ampère's theory, and furnishes powerful reasons for believing that the action is the same in both cases; but, as a distinction in language is still necessary, I propose to call the agency thus exerted by ordinary magnets, magneto-electric or magnelectric induction (26).

59. The only difference which powerfully strikes the attention as existing between volta-electric and magneto-electric induction, is the suddenness of the former, and the sensible time required by the latter: but even in this early state of investigation there are circumstances which seem to indicate, that upon further inquiry this difference will, as a philosophical distinction, disappear (68).

XXVII

JOSEPH HENRY

1797-1878

Born at Albany, New York, December 17, 1797, Joseph Henry prepared for the profession of medicine, but an appointment as an assistant engineer on the state road diverted his interests toward mechanics. In 1826 he was appointed instructor of physics at Albany Institute, now the Albany Boys Academy, where he conducted his first experiments in electricity. In 1828 he first produced a strong electro-magnet by winding fine insulated wire around a piece of soft iron, and soon succeeded in exciting his electro-magnet at a distance by the use of high intensity batteries made up of many cells. Demonstrating that the number of coils of fine wire about a magnet had as much influence as the intensity of the current and that after winding many coils around the soft iron magnet it could still be made magnetic, he suggested the principle which Morse later used in the telegraph. In 1832 he discovered that in a long conductor the primary current, by an induction upon itself, produced a number of secondary currents that greatly increased the intensity of the discharge.

He was appointed professor of natural philosophy at Princeton University in 1832 and became secretary of the Smithsonian Institution in 1846. He died in Washington, May 13, 1878.

ON THE PRODUCTION OF CURRENTS AND SPARKS OF ELECTRICITY FROM MAGNETISM *

Although the discoveries of Oersted, Arago, Faraday, and others, have placed the intimate connection of electricity and magnetism in a most striking point of view, and although the theory of Ampère has referred all the phenomena of both these departments of science to the

* Silliman's *American Journal of Science*, July, 1832, Vol. XXII, pp. 403-408; *Scientific Writings*, Vol. I., p. 73.

same general laws, yet until lately one thing remained to be proved by experiment, in order more fully to establish their identity ; namely, the possibility of producing electrical effects from magnetism. It is well known that surprising magnetic results can readily be obtained from electricity, and at first sight it might be supposed that electrical effects could with equal facility be produced from magnetism ; but such has not been found to be the case, for although the experiment has often been attempted, it has nearly as often failed.

It early occurred to me that if galvanic magnets on my plan were substituted for ordinary magnets, in researches of this kind, more success might be expected. Besides their great powers these magnets possess other properties, which render them important instruments in the hands of the experimenter ; their polarity can be instantaneously reversed, and their magnetism suddenly destroyed or called into full action, according as the occasion may require. With this view, I commenced, last August, the construction of a much larger galvanic magnet than, to my knowledge, had before been attempted, and also made preparations for a series of experiments with it on a large scale, in reference to the production of electricity from magnetism. I was, however, at that time accidentally interrupted in the prosecution of these experiments, and have not been able since to resume them until within the last few weeks, and then on a much smaller scale than was at first intended. In the meantime, it has been announced in the 117th number of the *Library of Useful Knowledge*, that the result so much sought after has at length been found by Mr. Faraday of the Royal Institution. It states that he has established the general fact, that when a piece of metal is moved in any direction, in front of a magnetic pole, electrical currents are developed in the metal, which pass in a direction at right angles to its own motion, and also that the application of this principle affords a complete and satisfactory explanation of the phenomena of magnetic rotation. No detail is given of the experiments, and it is somewhat surprising that results so interesting, and which certainly form a new era in the history of electricity and magnetism, should not have been more fully described before this time in some of the English publications ; the only mention I have found of them is the following short account from the *Annals of Philosophy* for April, under the head of Proceedings of the Royal Institution :

"Feb. 17.—Mr. Faraday gave an account of the first two parts of his researches in electricity; namely, Volta-electric induction and magneto-electric induction. If two wires, A and B, be placed side by side, but not in contact, and a Voltaic current be passed through A, there is instantly a current produced by induction in B, in the opposite direction. Although the principal current in A be continued, still the secondary current in B is not found to accompany it, for it ceases after the first moment, but when the principal current is stopped, then there is a second current produced in B, in the opposite direction to that of the first produced by the inductive action, or in the same direction as that of the principal current.

"If a wire, connected at both extremities with a galvanometer, be coiled in the form of a helix around a magnet, no current of electricity takes place in it. This is an experiment which has been made by various persons hundreds of times, in the hope of evolving electricity from magnetism, and in other cases in which the wishes of the experimenter and the facts are opposed to each other, has given rise to very conflicting conclusions. But if the magnet be withdrawn from or introduced into such a helix, a current of electricity is produced whilst the magnet is in motion, and is rendered evident by the deflection of the galvanometer. If a single wire be passed by a magnetic pole, a current of electricity is induced through it which can be rendered sensible."

Before having any knowledge of the method given in the above account, I had succeeded in producing electrical effects in the following manner, which differs from that employed by Mr. Faraday, and which appears to me to develop some new and interesting facts. A piece of copper wire, about thirty feet long and covered with elastic varnish, was closely coiled around the middle of the soft iron armature of the galvanic magnet described in Vol. XIX of the *American Journal of Science*, and which, when excited, will readily sustain between six hundred and seven hundred pounds. The wire was wound upon itself so as to occupy only about one inch of the length of the armature which is seven inches in all. The armature, thus furnished with the wire, was placed in its proper position across the ends of the galvanic magnet, and there fastened so that no motion could take place. The two protecting ends of the helix were dipped into two cups of mercury, and there connected with a distant galvanometer by means of two copper wires, each about forty feet long. This arrange-

ment being completed, I stationed myself near the galvanometer and directed an assistant at a given word to immerse suddenly, in a vessel of dilute acid, the galvanic battery attached to the magnet. At the instant of immersion, the north end of the needle was deflected 30° to the west, indicating a current of electricity from the helix surrounding the armature. The effect, however, appeared only as a single impulse, for the needle, after a few oscillations, resumed its former undisturbed position in the magnetic meridian, although the galvanic action of the battery, and consequently the magnetic power, was still continued. I was, however, much surprised to see the needle suddenly deflected from a state of rest to about 20° to the east, or in a contrary direction when the battery was withdrawn from the acid, and again deflected to the west when it was re-immersed. This operation was repeated many times in succession, and uniformly with the same result, the armature the whole time remaining immovably attached to the poles of the magnet, no motion being required to produce the effect, as it appeared to take place only in consequence of the instantaneous development of the magnetic action in one, and the sudden cessation of it in the other.

This experiment illustrates most strikingly the reciprocal action of the two principles of electricity and magnetism, if indeed it does not establish their absolute identity. In the first place, magnetism is developed in the soft iron of the galvanic magnet by the action of the currents of electricity from the battery, and secondly, the armature, rendered magnetic by contact with the poles of the magnet, induces in its turn currents of electricity in the helix which surrounds it; we have thus, as it were, electricity converted into magnetism and this magnetism again into electricity.

Another fact was observed which is somewhat interesting, inasmuch as it serves in some respects to generalize the phenomena. After the battery had been withdrawn from the acid, and the needle of the galvanometer suffered to come to a state of rest after the resulting deflection, it was again deflected in the same direction by partially detaching the armature from the poles of the magnet to which it continued to adhere from the action of the residual magnetism, and in this way, a series of deflections, all in the same direction, was produced by merely slipping off the armature by degrees until the contact

was entirely broken. The following extract from the register of the experiments exhibits the relative deflections observed in one experiment of this kind.

At the instant of immersion of the battery, deflection 40° west.

At the instant of emersion of the battery, deflection 18° east.

Armature partially detached, deflection 7° east.

Armature entirely detached, deflection 12° west.

The effect was reversed in another experiment, in which the needle was turned to the west in a series of deflections by dipping the battery but a small distance into the acid at first and afterwards immersing it by degrees.

From the foregoing facts it appears that a current of electricity is produced, for an instant, in a helix of copper wire surrounding a piece of soft iron whenever magnetism is induced in the iron; and a current in an opposite direction when the magnetic action ceases; also that an instantaneous current in one or the other direction accompanies every change in the magnetic intensity of the iron.

Since reading the account before given of Mr. Faraday's method of producing electrical currents I have attempted to combine the effects of motion and induction; for this purpose a rod of soft iron ten inches long and one inch and a quarter in diameter, was attached to a common turning lathe, and surrounded with four helices of copper wire in such a manner that it could be suddenly and powerfully magnetized, while in rapid motion, by transmitting galvanic currents through three of the helices; the fourth being connected with the distant galvanometer was intended to transmit the current of induced electricity; all the helices were stationary while the iron rod revolved on its axis within them. From a number of trials in succession, first with the rod in one direction, then in the opposite, and next in a state of rest, it was concluded that no perceptible effect was produced on the intensity of the magneto-electric current by a rotary motion of the iron combined with its sudden magnetization.

The same apparatus, however, furnished the means of measuring separately the relative power of motion and induction in producing electrical currents. The iron rod was first magnetized by currents through the helices attached to the battery and while in this state one of its ends was quickly introduced into the helix connected with the galvanometer; the deflection of the needle in this case was seven de-

grees. The end of the rod was next introduced into the same helix while in its natural state and then suddenly magnetized; the deflection in this instance amounted to thirty degrees, showing a great superiority in the method of induction.

The next attempt was to increase the magneto-electric effect while the magnetic power remained the same, and in this I was more successful. Two iron rods six inches long and one inch in diameter were each surrounded by two helices and then placed perpendicularly on the face of the armature, and between it and the poles of the magnet, so that each rod formed, as it were, a prolongation of the poles, and to these the armature adhered when the magnet was excited. With this arrangement, a current from one helix produced a deflection of thirty-seven degrees; from two helices both on the same rod, fifty-two degrees, and from three fifty-nine degrees; but when four helices were used, the deflection was only fifty-five degrees, and when to these were added the helix of smaller wire around the armature, the deflection was no more than thirty degrees. This result may perhaps have been somewhat affected by the want of proper insulation in the several spires of the helices; it, however, establishes the fact that an increase in the electric current is produced by using at least two or three helices instead of one. The same principle was applied to another arrangement which seems to afford the maximum of electric development from a given magnetic power; in place of the two pieces of iron and the armature used in the last experiments, the poles of the magnet were connected by a single rod of iron, bent into the form of a horse-shoe, and its extremities filed perfectly flat so as to come in perfect contact with the faces of the poles; around the middle of the arch of this horse-shoe, two strands of copper wire were tightly coiled one over the other. A current from one of these helices deflected the needle one hundred degrees, and when both were used the needle was deflected with such force as to make a complete circuit. But the most surprising effect was produced when, instead of passing the current through the long wires to the galvanometer, the opposite ends of the helices were held nearly in contact with each other, and the magnet suddenly excited; in this case a small but vivid spark was seen to pass between the ends of the wires, and this effect was repeated as often as the state of intensity of the magnet was changed.

In these experiments the connection of the battery with the wires

from the magnet was not formed by soldering, but by two cups of mercury, which permitted the galvanic action on the magnet to be instantaneously suspended and the polarity to be changed and rechanged without removing the battery from the acid; a succession of vivid sparks was obtained by rapidly interrupting and forming the communication by means of one of these cups; but the greatest effect was produced when the magnetism was entirely destroyed and instantaneously reproduced by a change of polarity.

It appears from the May number of the *Annals of Philosophy* that I have been anticipated in this experiment of drawing sparks from the magnet by Mr. James D. Forbes of Edinburgh, who obtained a spark on the 30th of March; my experiment being made during the last two weeks of June. A simple notification of his result is given, without any account of the experiment, which is reserved for a communication to the Royal Society of Edinburgh; my result is therefore entirely independent of his and was undoubtedly obtained by a different process.

ELECTRICAL SELF-INDUCTION IN A LONG HELICAL WIRE

I have made several other experiments in relation to the same subject, but which more important duties will not permit me to verify in time for this paper. I may, however, mention one fact which I have not seen noticed in any work, and which appears to me to belong to the same class of phenomena as those before described; it is this: when a small battery is moderately excited by diluted acid, and its poles, which should be terminated by cups of mercury, are connected by a copper wire not more than a foot in length, no spark is perceived when the connection is either formed or broken; but if a wire thirty or forty feet long be used instead of the short wire, though no spark will be perceptible when the connection is made, yet when it is broken by drawing one end of the wire from its cup of mercury, a vivid spark is produced. If the action of the battery be very intense, a spark will be given by the short wire; in this case it is only necessary to wait a few minutes until the action partially subsides, and until no more sparks are given from the short wire; if the long wire be now substituted a spark will again be obtained. The effect appears somewhat increased by coiling the wire into a helix; it seems also to depend in

some measure on the length and thickness of the wire. I can account for these phenomena only by supposing the long wire to become charged with electricity, which by its re-action on itself projects a spark when the connection is broken.

XXVIII

SIR CHARLES LYELL

1797-1875

Sir Charles Lyell, the son of a Scottish botanist of literary tastes, was born at Kinnordy, Scotland, November 14, 1797. He went to Oxford University, from which he graduated in 1819. He was admitted to the bar in 1825. In 1827 he abandoned law for geology, and published his "Principles of Geology" in 1830-1833. Lyell's thesis was that all the past changes of the earth were explainable by forces now operative—an idea which underlies modern geology. He published his "Antiquity of Man" in 1863, providing proofs of man's long existence on earth and thus contributing to the establishment of the Darwinian theory. He died February 22, 1875.

UNIFORMITY IN THE SERIES OF PAST CHANGES IN THE ANIMATE AND INANIMATE WORLD *

Origin of the doctrine of alternate periods of repose and disorder.—It has been truly observed that when we arrange the fossiliferous formations in chronological order, they constitute a broken and defective series of monuments; we pass without any intermediate gradations from systems of strata which are horizontal, to other systems which are highly inclined—from rocks of peculiar mineral composition to others which have a character wholly distinct—from one assemblage of organic remains to another, in which frequently nearly all the species, and a large part of the genera, are different. These violations of continuity are so common as to constitute in most regions the rule rather than the exception, and they have been considered by many geologists as conclusive in favour of sudden revolutions in the inanimate and animate world. We have already seen that according to

* From the *Principles of Geology*, Bk. I, Ch. XIII.

the speculations of some writers, there have been in the past history of the planet alternate periods of tranquility and convulsion, the former enduring for ages, and resembling the state of things now experienced by man; the other brief, transient, and paroxysmal, giving rise to new mountains, seas, and valleys, annihilating one set of organic beings and ushering in the creation of another.

It will be the object of the present chapter to demonstrate that these theoretical views are not borne out by a fair interpretation of geological monuments. It is true that in the solid framework of the globe we have a chronological chain of natural records, many links of which are wanting: but a careful consideration of all the phenomena leads to the opinion that the series was originally defective—that it has been rendered still more so by time—that a great part of what remains is inaccessible to man, and even of that fraction which is accessible nine-tenths or more are to this day unexplored.

The readiest way, perhaps, of persuading the reader that we may dispense with great and sudden revolutions in the geological order of events is by showing him how a regular and uninterrupted series of changes in the animate and inanimate world must give rise to such breaks in the sequence, and such unconformability of stratified rocks, as are usually thought to imply convulsions and catastrophes. It is scarcely necessary to state that the order of events thus assumed to occur, for the sake of illustration, should be in harmony with all the conclusions legitimately drawn by geologists from the structure of the earth, and must be equally in accordance with the changes observed by man to be now going on in the living as well as in the inorganic creation. It may be necessary in the present state of science to supply some part of the assumed course of nature hypothetically; but if so, this must be done without any violation of probability, and always consistently with the analogy of what is known both of the past and present economy of our system. Although the discussion of so comprehensive a subject must carry the beginner far beyond his depth, it will also, it is hoped, stimulate his curiosity, and prepare him to read some elementary treatises on geology with advantage, and teach him the bearing on that science of the changes now in progress on the earth. At the same time it may enable him the better to understand the intimate connection between the Second and Third Books of this work, one of which is occupied with the changes

of the inorganic, the latter with those of the organic creation.

In pursuance, then, of the plan above proposed, I will consider in this chapter, first, the laws which regulate the denudation of strata and the deposition of sediment; secondly, those which govern the fluctuation in the animate world; and thirdly, the mode in which subterranean movements affect the earth's crust.

Uniformity of change considered, first, in reference to denudation and sedimentary deposition.—First, in regard to the laws governing the deposition of new strata. If we survey the surface of the globe, we immediately perceive that it is divisible into areas of deposition and non-deposition; or, in other words, at any given time there are spaces which are the recipients, others which are not the recipients, of sedimentary matter. No new strata, for example, are thrown down on dry land, which remains the same from year to year; whereas, in many parts of the bottom of seas and lakes, mud, sand, and pebbles are annually spread out by rivers and currents. There are also great masses of limestone growing in some seas, chiefly composed of corals and shells, or, as in the depths of the Atlantic, of chalky mud made up of foraminifera and diatomaceæ.

As to the dry land, so far from being the receptacle of fresh accessions of matter, it is exposed almost everywhere to waste away. Forests may be as dense and lofty as those of Brazil, and may swarm with quadrupeds, birds, and insects, yet at the end of thousands of years one layer of black mould a few inches thick may be the sole representative of those myriads of trees, leaves, flowers, and fruits, those innumerable bones and skeletons of birds, quadrupeds, and reptiles, which tenanted the fertile region. Should this land be at length submerged, the waves of the sea may wash away in a few hours the scanty covering of mould, and it may merely import a darker shade of colour to the next stratum of marl, sand, or other matter newly thrown down. So also at the bottom of the ocean where no sediment is accumulating, seaweed, zoophytes, fish, and even shells, may multiply for ages and decompose, leaving no vestige of their form or substance behind. Their decay, in water, although more slow, is as certain and eventually as complete as in the open air. Nor can they be perpetuated for indefinite periods in a fossil state, unless imbedded in some matrix which is impervious to water, or which at least does not allow a free percolation of that fluid, impregnated as it usually is, with a

slight quantity of carbonic or other acid. Such a free percolation may be prevented either by the mineral nature of the matrix itself, or by the superposition of an impermeable stratum; but if unimpeded, the fossil shell or bone will be dissolved and removed, particle after particle, and thus entirely effaced, unless petrification or the substitution of some mineral for the organic matter happen to take place.

That there has been land as well as sea at all former geological periods, we know from the fact that fossil trees and terrestrial plants are imbedded in rocks of every age, except those which are so ancient as to be very imperfectly known to us. Occasionally lacustrine and fluviatile shells, or the bones of amphibious or land reptiles, point to the same conclusion. The existence of dry land at all periods of the past implies, as before mentioned, the partial deposition of sediment, or its limitation to certain areas; and the next point to which I shall call the reader's attention is the shifting of these areas from one region to another.

First, then, variations in the site of sedimentary deposition are brought about independently of subterranean movements. There is always a slight change from year to year, or from century to century. The sediment of the Rhone, for example, thrown in the Lake of Geneva, is now conveyed to a spot a mile and a half distant from that where it accumulated in the tenth century, and six miles from the point where the delta began originally to form. We may look forward to the period when this lake will be filled up, and then the distribution of the transported matter will be suddenly altered, for the mud and sand brought down from the Alps will thenceforth, instead of being deposited near Geneva, be carried nearly 200 miles southwards, where the Rhone enters the Mediterranean.

In the deltas of large rivers, such as those of the Ganges and Indus, the mud is first carried down for many centuries through one arm, and on this being stopped up it is discharged by another, and may then enter the sea at a point 50 or 100 miles distant from its first receptacle. The direction of marine currents is also liable to be changed by various accidents, as by the heaping up of new sandbanks, or the wearing away of cliffs and promontories.

But, secondly, all these causes of fluctuation in the sedimentary areas are entirely subordinate to those great upward or downward

movements of lands, which will be presently spoken of, as prevailing over large tracts of the globe. By such elevation or subsidence certain spaces are gradually submerged, or made gradually to emerge: in the one case sedimentary deposition may be suddenly renewed after having been suspended for one or more geological periods, in the other as suddenly made to cease after having continued for ages.

If deposition be renewed after a long interval, the new strata will usually differ greatly from the sedimentary rocks previously formed in the same place, and especially if the older rocks have suffered derangement, which implies a change in the physical geography of the district since the previous conveyance of sediment to the same spot. It may happen, however, that, even where the two groups, the superior and the inferior, are horizontal and conformable to each other, they may still differ entirely in mineral character, because, since the origin of the older formation, the geography of some distant country has been altered. In that country rocks before concealed may have become exposed by denudation; volcanoes may have burst out and covered the surface with scoriæ and lava; or new lakes, intercepting the sediment previously conveyed from the upper country, may have been formed by subsidence; and other fluctuations may have occurred, by which the materials brought down from thence by rivers to the sea have acquired a distinct mineral character.

It is well known that the stream of the Mississippi is charged with sediment of a different colour from that of the Arkansas and Red Rivers, which are tinged with red mud, derived from rocks of porphyry and red gypseous clays in "the far west." The waters of the Uruguay, says Darwin, draining a granitic country, are clear and black, those of the Parana, red. The mud with which the Indus is loaded, says Burnes, is of a clayey hue, that of the Chenab, on the other hand, is reddish, that of the Sutlej is more pale. The same causes which make these several rivers, sometimes situated at no great distance the one from the other, to differ greatly in the character of their sediment, will make the waters draining the same country at different epochs, especially before and after great revolutions in physical geography, to be entirely dissimilar. It is scarcely necessary to add that marine currents will be affected in an analogous manner in consequence of the formation of new shoals, the emergence of new islands, the subsidence of others, the gradual waste of neighbouring

coasts, the growth of new deltas, the increase of coral reefs, volcanic eruptions, and other changes.

Uniformity of change considered, secondly, in reference to the living creation.—Secondly, in regard to the vicissitudes of the living creation, all are agreed that the successive groups of sedimentary strata found in the earth's new crust are not only dissimilar in mineral composition for reasons above alluded to, but are likewise distinguishable from each other by their organic remains. The general inference drawn from the study and comparison of the various groups, arranged in chronological order, is this: that at successive periods distinct tribes of animals and plants have inhabited the land and waters, and that the organic types of the newer formations are more analogous to species now existing than those of more ancient rocks. If we then turn to the present state of the animate creation, and inquire whether it has now become fixed and stationary, we discover that, on the contrary, it is in a state of continual flux—that there are many causes in action which tend to the extinction of species, and which are conclusive against the doctrine of their unlimited durability.

There are also causes which give rise to new varieties and races in plants and animals, and new forms are continually supplanting others which had endured for ages. But natural history has been successfully cultivated for so short a period, that a few examples only of local, and perhaps but one or two of absolute, extirpation of species can as yet be proved, and these only where the interference of man has been conspicuous. It will nevertheless appear evident, from the facts and arguments detailed in the chapters which treat of the geographical distribution of species in the next volume, that man is not the only exterminating agent; and that, independently of his intervention, the annihilation of species is promoted by the multiplication and gradual diffusion of every animal or plant. It will also appear that every alteration in the physical geography and climate of the globe cannot fail to have the same tendency. If we proceed still farther, and inquire whether new species are substituted from time to time for those which die out, we find that the successive introduction of new forms appears to have been a constant part of the economy of the terrestrial system, and if we have no direct proof of the fact it is because the changes take place so slowly as not to come within the period of exact scientific observation. To enable the reader to appre-

ciate the gradual manner in which a passage may have taken place from an extinct fauna to that now living, I shall say a few words on the fossils of successive Tertiary periods. When we trace the series of formations from the more ancient to the more modern, it is in these Tertiary deposits that we first meet with assemblages of organic remains having a near analogy to the fauna of certain parts of the globe in our own time. In the Eocene, or oldest subdivisions, some few of the testacea belong to existing species, although almost all of them, and apparently all the associated vertebrata, are now extinct. These Eocene strata are succeeded by a great number of more modern deposits, which depart gradually in the character of their fossils from the Eocene type, and approach more and more to that of the living creation. In the present state of science, it is chiefly by the aid of shells, that we are enabled to arrive at these results, for of all classes the testacea are the most generally diffused in a fossil state, and may be called the medals principally employed by nature in recording the chronology of past events. In the Upper Miocene rocks (No. 5 of the table, p. 135) we begin to find a considerable number, although still a minority, of recent species, intermixed with some fossils common to the preceding, or Eocene, epoch. We then arrive at the Pliocene strata, in which species now contemporary with man begin to preponderate, and in the newest of which nine-tenths of the fossils agree with species still inhabiting the neighbouring sea. It is in the Post-Tertiary strata, where all the shells agree with species now living, that we have discovered the first or earliest known remains of man associated with the bones of quadrupeds, some of which are of extinct species.

In thus passing from the older to the newer members of the Tertiary system, we meet with many chasms, but none which separate entirely, by a broad line of demarcation, one state of the organic world from another. There are no signs of an abrupt termination of one fauna and flora, and the starting into life of new and wholly distinct forms. Although we are far from being able to demonstrate geologically an insensible transition from the Eocene to the Miocene, or even from the latter to the recent fauna, yet the more we enlarge and perfect our general survey, the more nearly do we approximate to such a continuous series, and the more gradually are we conducted from times when many of the genera and nearly all the species were extinct,

to those in which scarcely a single species flourished, which we do not know to exist at present. Dr. A. Philippi, indeed, after an elaborate comparison of the fossil tertiary shells of Sicily with those now living in the Mediterranean, announced, as the result of his examination, that there are strata in that island which attest a very gradual passage from a period when only thirteen in a hundred of the shells were like the species now living in the sea, to an era when the recent species had attained a proportion of ninety-five in a hundred. There is, therefore, evidence, he says, in Sicily of this revolution in the animate world having been effected "without the intervention of any convulsion or abrupt changes, certain species having from time died out, and others having been introduced, until at length the existing fauna was elaborated."

In no part of Europe is the absence of all signs of man or his works, in strata of comparatively modern date, more striking than in Sicily. In the central parts of that island we observe a lofty table-land and hills, sometimes rising to the height of 3,000 feet, capped with a limestone, in which from 70 to 85 per cent of the fossil testacea are specifically identical with those now inhabiting the Mediterranean. These calcareous and other argillaceous strata of the same age are intersected by deep valleys which appear to have been gradually formed by denudation, but have not varied materially in width or depth since Sicily was first colonized by the Greeks. The limestone, moreover, which is of so late a date in geological chronology, was quarried for building those ancient temples of Girgenti and Syracuse, of which the ruins carry us back to a remote era in human history. If we are lost in conjectures when speculating on the ages required to lift up these formations to the height of several thousand feet above the sea, and to excavate the valleys, how much more remote must be the era when the same rocks were gradually formed beneath the waters!

The intense cold of the Glacial period was spoken of in the tenth chapter. Although we have not yet succeeded in detecting proofs of the origin of man antecedently to that epoch, we have yet found evidence that most of the testacea, and not a few of the quadrupeds, which preceded, were of the same species as those which followed the extreme cold. To whatever local disturbances this cold may have given rise in the distribution of species, it seems to have done little in effecting their annihilation. We may conclude, therefore, from a

survey of the tertiary and modern strata, which constitute a more complete and unbroken series than rocks of older date, that the extinction and creation of species have been, and are, the result of a slow and gradual change in the organic world.

Uniformity of change considered, thirdly, in reference to subterranean movements.—Thirdly, to pass on to the last of the three topics before proposed for discussion, the reader will find, in the account given in the Second Book, Vol. II., of the earthquakes recorded in history, that certain countries have, from time immemorial, been rudely shaken again and again; while others, comprising by far the largest part of the globe, have remained to all appearance motionless. In the regions of convulsion rocks have been rent asunder, the surface has been forced up into ridges, chasms have opened, or the ground throughout large spaces has been permanently lifted up above or let down below its former level. In the regions of tranquillity some areas have remained at rest, but others have been ascertained, by a comparison of measurements made at different periods, to have arisen by an insensible motion, as in Sweden, or to have subsided very slowly, as in Greenland. That these same movements, whether ascending or descending, have continued for ages in the same direction has been established by historical or geological evidence. Thus we find on the opposite coasts of Sweden that brackish water deposits, like those now forming in the Baltic, occur on the eastern side, and upraised strata filled with purely marine shells, now proper to the ocean, on the western coast. Both of these have been lifted up to an elevation of several hundred feet above high-water mark. The rise within the historical period has not amounted to many yards, but the greater extent of antecedent upheaval is proved by the occurrence in inland spots, several hundred feet high, of deposits filled with fossil shells of species now living either in the ocean or the Baltic.

It must in general be more difficult to detect proofs of slow and gradual subsidence than of elevation, but the theory which accounts for the form of circular coral reefs and lagoon islands, and which will be explained in the concluding chapter of this work, will satisfy the reader that there are spaces on the globe, several thousand miles in circumference, throughout which the downward movement has predominated for ages, and yet the land has never, in a single instance, gone down suddenly for several hundred feet at once. Yet geology

demonstrates that the persistency of subterranean movements in one direction has not been perpetual throughout all past time. There have been great oscillations of level, by which a surface of dry land has been submerged to a depth of several thousand feet, and then at a period long subsequent raised again and made to emerge. Nor have the regions now motionless been always at rest; and some of those which are at present the theatres of reiterated earthquakes have formerly enjoyed a long continuance of tranquillity. But, although disturbances have ceased after having long prevailed, or have recommenced after a suspension of ages, there has been no universal disruption of the earth's crust or desolation of the surface since times the most remote. The non-occurrence of such a general convulsion is proved by the perfect horizontality now retained by some of the most ancient fossiliferous strata throughout wide areas.

That the subterranean forces have visited different parts of the globe at successive periods is inferred chiefly from the unconformability of strata belonging to groups of different ages. Thus, for example, on the borders of Wales and Shropshire, we find the slaty beds of the ancient Silurian system inclined and vertical, while the beds of the overlying carboniferous shale and sandstone are horizontal. All are agreed that in such a case the older set of strata had suffered great disturbance before the deposition of the newer or carboniferous beds, and that these last have never since been violently fractured, nor have ever been bent into folds, whether by sudden or continuous lateral pressure. On the other hand, the more ancient or Silurian group suffered only a local derangement, and neither in Wales nor elsewhere are all the rocks of that age found to be curved or vertical.

In various parts of Europe, for example, and particularly near Lake Wener in the south of Sweden, and in many parts of Russia, the Silurian strata maintain the most perfect horizontality; and a similar observation may be made respecting limestones and shales of like antiquity in the great lake district of Canada and the United States. These older rocks are still as flat and horizontal as when first formed; yet, since their origin, not only have most of the actual mountain-chains been uplifted, but some of the very rocks of which those mountains are composed have been formed, some of them by igneous and others by aqueous action.

It would be easy to multiply instances of similar unconformability

in formations of other ages ; but a few more will suffice. The carboniferous rocks before alluded to as horizontal on the borders of Wales are vertical in the Mendip hills in Somersetshire, where the overlying beds of the New Red Sandstone are horizontal. Again, in the Wolds of Yorkshire the last-mentioned sandstone supports on its curved and inclined beds the horizontal Chalk. The Chalk again is vertical on the flanks of the Pyrenees, and the tertiary strata repose unconformably upon it.

As almost every country supplies illustrations of the same phenomena, they who advocate the doctrine of alternate periods of disorder and repose may appeal to the facts above described, as proving that every district has been by turns convulsed by earthquakes and then respited for ages from convulsions. But so it might with equal truth be affirmed that every part of Europe has been visited alternately by winter and summer, although it has always been winter and always summer in some part of the planet, and neither of these seasons has ever reigned simultaneously over the entire globe. They have been always shifting from place to place ; but the vicissitudes which recur thus annually in a single spot are never allowed to interfere with the invariable uniformity of seasons throughout the whole planet.

So, in regard to subterranean movements, the theory of the perpetual uniformity of the force which they exert on the earth's crust is quite consistent with the admission of their alternate development and suspension for long and indefinite periods within limited geographical areas.

If, for reasons before stated, we assume a continual extinction of species and appearance of others on the globe, it will then follow that the fossils of strata formed at two distant periods on the same spot will differ even more certainly than the mineral composition of those strata. For rocks of the same kind have sometimes been reproduced in the same district after a long interval of time ; whereas all the evidence derived from fossil remains is in favour of the opinion that species which have once died out have never been reproduced. The submergence, then, of land must be often attended by the commencement of a new class of sedimentary deposits, characterized by a new set of fossil animals and plants, while the reconversion of the bed of the sea into land may arrest at once and for an indefinite time the formation of geological monuments. Should the land again sink,

strata will again be formed; but one or many entire revolutions in animal or vegetable life may have been completed in the interval.

As to the want of completeness in the fossiliferous series, which may be said to be almost universal, we have only to reflect on what has been already said of the laws governing sedimentary deposition, and those which give rise to fluctuations in the animate world, to be convinced that a very rare combination of circumstances can alone give rise to such a superposition and preservation of strata as will bear testimony to the gradual passage from one state of organic life to another. To produce such strata nothing less will be requisite than the fortunate coincidence of the following conditions: first, a never-failing supply of sediment in the same region throughout a period of vast duration; secondly, the fitness of the deposit in every part for the permanent preservation of imbedded fossils; and, thirdly, a gradual subsidence to prevent the sea or lake from being filled up and converted into land.

It will appear in the chapter on coral reefs, that, in certain parts of the Pacific and Indian Oceans, most of these conditions, if not all, are complied with, and the constant growth of coral, keeping pace with the sinking of the bottom of the sea, seems to have gone on so slowly, for such indefinite periods, that the signs of a gradual change in organic life might probably be detected in that quarter of the globe if we could explore its submarine geology. Instead of the growth of coralline limestone, let us suppose, in some other place, the continuous deposition of fluvatile mud and sand, such as the Ganges and Brahmapootra have poured for thousands of years into the Bay of Bengal. Part of this bay, although of considerable depth, might at length be filled up before an appreciable amount of change was effected in the fish, mollusca, and other inhabitants of the sea and neighbouring land. But if the bottom be lowered by sinking at the same rate that it is raised by fluvatile mud, the bay can never be turned into dry land. In that case one new layer of matter may be superimposed upon another for a thickness of many thousand feet, and the fossils of the inferior beds may differ greatly from those entombed in the uppermost, yet every intermediate gradation may be indicated in the passage from an older to a newer assemblage of species. Granting, however, that such an unbroken sequence of monuments may thus be elaborated in certain parts of the sea, and

that the strata happen to be all of them well adapted to preserve the included fossils from decomposition, how many accidents must still concur before these submarine formations will be laid open to our investigation! The whole deposit must first be raised several thousand feet, in order to bring into view the very foundation; and during the process of exposure the superior beds must not be entirely swept away by denudation.

In the first place, the chances are nearly as three to one against the mere emergence of the mass above the waters, because nearly three-fourths of the globe are covered by the ocean. But if it be upheaved and made to constitute part of the dry land, it must also, before it can be available for our instruction, become part of that area already surveyed by geologists. In this small fraction of land already explored, and still very imperfectly known, we are required to find a set of strata deposited under peculiar conditions, and which, having been originally of limited extent, would have been probably much lessened by subsequent denudation.

Yet it is precisely because we do not encounter at every step the evidence of such gradations from one state of the organic world to another, that so many geologists have embraced the doctrine of great and sudden revolutions in the history of the animate world. Not content with simply availing themselves, for the convenience of classification, of those gaps and chasms which here and there interrupt the continuity of the chronological series, as at present known, they deduce, from the frequency of these breaks in the chain of records, an irregular mode of succession in the events themselves, both in the organic and inorganic world. But, besides that some links of the chain which once existed are now entirely lost and others concealed from view, we have good reason to suspect that it was never complete originally. It may undoubtedly be said that strata have been always forming somewhere, and therefore at every moment of past time Nature has added a page to her archives; but, in reference to this subject, it should be remembered that we can never hope to compile a consecutive history by gathering together monuments which were originally detached and scattered over the globe. For, as the species of organic beings contemporaneously inhabiting remote regions are distinct, the fossils of the first of several periods which may be preserved in any one country, as in America for example, will have no

connection with those of a second period found in India, and will therefore no more enable us to trace the signs of a gradual change in the living creation, than a fragment of Chinese history will fill up a blank in the political annals of Europe.

The absence of any deposits of importance containing recent shells in Chili, or anywhere on the western shore of South America, naturally led Mr. Darwin to the conclusion that "where the bed of the sea is either stationary or rising, circumstances are far less favourable than where the level is sinking to the accumulation of conchiferous strata of sufficient thickness and extension to resist the average vast amount of denudation." In like manner the beds of superficial sand, clay, and gravel, with recent shells, on the coasts of Norway and Sweden, where the land has risen in Post-tertiary times, are so thin and scanty as to incline us to admit a similar proposition. We may in fact assume that in all cases where the bottom of the sea has been undergoing continuous elevation, the total thickness of sedimentary matter accumulating at depths suited to the habitation of most of the species of shells can never be great, nor can the deposits be thickly covered with superincumbent matter, so as to be consolidated by pressure. When they are upheaved, therefore, the waves on the beach will bear down and disperse the loose materials; whereas, if the bed of the sea subsides slowly, a mass of strata containing abundance of such species as live at moderate depths, may be formed and may increase in thickness to any amount. It may also extend horizontally over a broad area, as the water gradually encroaches on the subsiding land.

Hence it will follow that great violations of continuity in the chronological series of fossiliferous rocks will always exist, and the imperfection of the record, though lessened, will never be removed by future discoveries. For not only will no deposits originate on the dry land, but those formed in the sea near land, which is undergoing constant upheaval, will usually be too slight in thickness to endure for ages.

In proportion as we become acquainted with larger geographical areas, many of the gaps, by which a chronological table is rendered defective, will be removed. We were enabled by aid of the labours of Prof. Sedgwick and Sir Roderick Murchison, to intercalate, in 1838, the marine strata of the Devonian period, with their fossil

shells, corals, and fish, between the Silurian and Carboniferous rocks. Previously the marine fauna of these last-mentioned formations wanted the connecting links which now render the passage from the one to the other much less abrupt. In like manner the Upper Miocene has no representative in England, but in France, Germany, and Switzerland it constitutes a most instructive link between the living creation and the middle of the great Tertiary period. Still we must expect, for reasons before stated, that chasms will forever continue to occur, in some parts of our sedimentary series.

Concluding remarks on the consistency of the theory of gradual change with the existence of great breaks in the series.—To return to the general argument pursued in this chapter, it is assumed, for reasons above explained, that a slow change of species is in simultaneous operation everywhere throughout the habitable surface of sea and land; whereas the fossilization of plants and animals is confined to those areas where new strata are produced. These areas, as we have seen, are always shifting their position, so that the fossilizing process, by means of which the commemoration of the particular state of the organic world, at any given time, is effected, may be said to move about, visiting and revisiting different tracts in succession.

To make still more clear the supposed working of this machinery, I shall compare it to a somewhat analogous case that might be imagined to occur in the history of human affairs. Let the mortality of the population of a large country represent the successive extinction of species, and the births of new individuals the introduction of new species. While these fluctuations are gradually taking place everywhere, suppose commissioners to be appointed to visit each province of the country in succession, taking an exact account of the number, names and individual peculiarities of all the inhabitants, and leaving in each district a register containing a record of this information. If, after the completion of one census, another is immediately made on the same plan, and then another, there will at last be a series of statistical documents in each province. When those belonging to any one province are arranged in chronological order, the contents of such as stand next to each other will differ according to the length of the intervals of time between the taking of each census. If, for example, there are sixty provinces, and all the registers are made in a single year and renewed annually, the number of births and deaths

will be so small, in proportion to the whole of the inhabitants, during the interval between the compiling of two consecutive documents, that the individuals described in such documents will be nearly identical; whereas, if the survey of each of the sixty provinces occupies all the commissioners for a whole year, so that they are unable to revisit the same place until the expiration of sixty years, there will then be an almost entire discordance between the persons enumerated in two consecutive registers in the same province. There are, undoubtedly, other causes, besides the mere quantity of time, which may augment or diminish the amount of discrepancy. Thus, at some periods, a pestilential disease may have lessened the average duration of human life; or a variety of circumstances may have caused the births to be unusually numerous, and the population to multiply; or a province may be suddenly colonized by persons migrating from surrounding districts.

These exceptions may be compared to the accelerated rate of fluctuations in the fauna and flora of a particular region, in which the climate and physical geography may be undergoing an extraordinary degree of alteration.

But I must remind the reader that the case above proposed has no pretensions to be regarded as an exact parallel to the geological phenomena which I desire to illustrate; for the commissioners are supposed to visit the different provinces in rotation; whereas the commemorating processes by which organic remains become fossilized, although they are always shifting from one area to the other, are yet very irregular in their movements. They may abandon and revisit many spaces again and again, before they once approach another district; and, besides this source of irregularity, it may often happen that, while the depositing process is suspended, denudation may take place, which may be compared to the occasional destruction by fire or other causes of some of the statistical documents before mentioned. It is evident that where such accidents occur the want of continuity in the series may become indefinitely great, and that the monuments which follow next in succession will by no means be equidistant from each other in point of time.

If this train of reasoning be admitted, the occasional distinctness of the fossil remains, in formations immediately in contact, would be a necessary consequence of the existing laws of sedimentary deposi-

tion and subterranean movement, accompanied by a constant dying-out and renovation of species.

As all the conclusions above insisted on are directly opposed to opinions still popular, I shall add another comparison, in the hope of preventing any possible misapprehension of the argument. Suppose we had discovered two buried cities at the foot of Vesuvius, immediately superimposed upon each other, with a great mass of tuff and lava intervening, just as Portici and Resina, if now covered with ashes, would overlies Herculaneum. An antiquary might possibly be entitled to infer, from the inscriptions on public edifices, that the inhabitants of the inferior and older city were Greeks, and those of the modern town Italians. But he would reason very hastily if he also concluded from these data, that there had been a sudden change from the Greek to the Italian language in Campania. But if he afterwards found three buried cities, one above the other, the intermediate one being Roman, while, as in the former example, the lowest was Greek and the uppermost Italian, he would then perceive the fallacy of his former opinion and would begin to suspect that the catastrophes, by which the cities were inhumed, might have no relation whatever to the fluctuations in the language of the inhabitants; and that, as the Roman tongue had evidently intervened between the Greek and Italian, so many other dialects may have been spoken in succession, and the passage from the Greek to the Italian may have been very gradual, some terms growing obsolete, while others were introduced from time to time.

If this antiquary could have shown that the volcanic paroxysms of Vesuvius were so governed as that cities should be buried one above the other, just as often as any variation occurred in the language of the inhabitants, then, indeed, the abrupt passage from a Greek to a Roman, and from a Roman to an Italian city, would afford proof of fluctuations no less sudden in the language of the people.

So, in Geology, if we could assume that it is part of the plan of Nature to preserve, in every region of the globe, an unbroken series of monuments to commemorate the vicissitudes of the organic creation, we might infer the sudden extirpation of species, and the simultaneous introduction of others, as often as two formations in contact are found to include dissimilar organic fossils. But we must shut our eyes to the whole economy of the existing causes, aqueous, igneous, and

organic, if we fail to perceive that such is not the plan of Nature.

I shall now conclude the discussion of a question with which we have been occupied since the beginning of the fifth chapter—namely, whether there has been any interruption, from the remotest periods, of one uniform and continuous system of change in the animate and inanimate world. We were induced to enter into that inquiry by reflecting how much the progress of opinion in Geology had been influenced by the assumption that the analogy was slight in kind, and still more slight in degree, between the causes which produced the former revolutions of the globe, and those now in every-day operation. It appeared clear that the earlier geologists had not only a scanty acquaintance with existing changes, but were singularly unconscious of the amount of their ignorance. With the presumption naturally inspired by this unconsciousness, they had no hesitation in deciding at once that time could never enable the existing powers of nature to work out changes of great magnitude, still less such important revolutions as those which are brought to light by Geology. They therefore felt themselves at liberty to indulge their imaginations in guessing at what might be, rather than inquiring what is; in other words, they employed themselves in conjecturing what might have been the course of Nature at a remote period, rather than in the investigation of what was the course of Nature in their own times.

It appeared to them far more philosophical to speculate on the possibilities of the past, than patiently to explore the realities of the present; and having invented theories under the influences of such maxims, they were consistently unwilling to test their validity by the criterion of their accordance with the ordinary operations of Nature. On the contrary, the claims of each new hypothesis to credibility appeared enhanced by the great contrast, in kind or intensity, of the causes referred to and those now in operation.

Never was there a dogma more calculated to foster indolence, and to blunt the keen edge of curiosity, than this assumption of the discordance between the ancient and existing causes of change. It produced a state of mind unfavourable in the highest degree to the candid reception of the evidence of those minute but incessant alterations which every part of the earth's surface is undergoing, and by which the condition of its living inhabitants is continually made to vary. The student, instead of being encouraged with the hope of interpret-

ing the enigmas presented to him in the earth's structure—instead of being prompted to undertake laborious inquiries into the natural history of the organic world, and the complicated effects of the igneous and aqueous causes now in operation—was taught to despond from the first. Geology, it was affirmed, could never rise to the rank of an exact science; the greater number of phenomena must forever remain inexplicable, or only be partially elucidated by ingenious conjectures. Even the mystery which invested the subject was said to constitute one of its principal charms, affording, as it did, full scope to the fancy to indulge in a boundless field of speculation.

The course directly opposed to this method of philosophizing consists in an earnest and patient inquiry, how far geological appearances are reconcilable with the effect of changes now in progress, or which may be in progress in regions inaccessible to us, but of which the reality is attested by volcanoes and subterranean movements. It also endeavours to estimate the aggregate result of ordinary operations multiplied by time, and cherishes a sanguine hope that the resources to be derived from observation and experiment, or from the study of Nature such as she now is, are very far from being exhausted. For this reason all theories are rejected which involve the assumption of sudden and violent catastrophes and revolutions of the whole earth, and its inhabitants—theories which are restrained by no reference to existing analogies, and in which a desire is manifested to cut, rather than patiently to untie, the Gordian knot.

We have now, at least, the advantage of knowing, from experience, that an opposite method has always put geologists on the road that leads to truth—suggesting views which, although imperfect at first, have been found capable of improvement, until at last adopted by universal consent; while the method of speculating on a former distinct state of things and causes has led invariably to a multitude of contradictory systems, which have been overthrown one after the other—have been found incapable of modification—and which have often required to be precisely reversed.

The remainder of this work will be devoted to an investigation of the changes now going on in the crust of the earth and its inhabitants. The importance which the student will attach to such researches will mainly depend on the degree of confidence which he feels in the principles above expounded. If he firmly believes in the resemblance or

identity of the ancient and present system of terrestrial changes, he will regard every fact collected respecting the causes in diurnal action as affording him a key to the interpretation of some mystery in the past. Events which have occurred at the most distant periods in the animate and inanimate world will be acknowledged to throw light on each other, and the deficiency of our information respecting some of the most obscure parts of the present creation will be removed. For as, by studying the external configuration of the existing land and its inhabitants, we may restore in imagination the appearance of the ancient continents which have passed away, so may we obtain from the deposits of ancient seas and lakes an insight into the nature of the subaqueous processes now in operation, and of many forms of organic life which, though now existing, are veiled from sight. Rocks, also, produced by subterranean fire in former ages, at great depths in the bowels of the earth, present us, when upraised by gradual movements, and exposed to the light of heaven, with an image of those changes which the deep-seated volcano may now occasion in the nether regions. Thus, although we are mere sojourners on the surface of the planet, chained to a mere point in space, enduring but for a moment of time, the human mind is not only enabled to number worlds beyond the unassisted ken of mortal eye, but to trace the events of indefinite ages before the creation of our race, and is not even withheld from penetrating into the dark secrets of the ocean, or the interior of the solid globe; free, like the spirit which the poet described as animating the universe,

—*ire per omnes*

Terrasque, tractusque maris, coelumque profundum.

XXIX

CHARLES DARWIN

1809-1882

Charles Robert Darwin, the grandson of Erasmus Darwin, was born at Shrewsbury, England, February 12, 1809. He studied at both Edinburgh and Cambridge, and graduated from the latter in 1831. From 1831 to 1836 he served as a naturalist on the "Beagle," which made a trip around the world in the interests of science. The voyage served as a post-graduate course for Darwin, who then first adopted his evolutionary ideas and developed as an original investigator. Reading Malthus, in 1838, on the problem of population and the food supply, he integrated Malthus' ideas into his own views of biology. In 1844 he began his "Origin of Species," which he completed in 1859. In 1858 he received a paper from Alfred Russell Wallace, then in the Malay Archipelago, which proposed the same theory of natural selection. Darwin believed that when organisms increased much faster than the means of subsistence, the ratios varied, and in the conditions produced by these natural causes only those organisms survived which were best fitted to their environment. He applied his concept to human evolution in his "Descent of Man," published in 1871. He died April 19, 1882, and was buried in Westminster Abbey.

NATURAL SELECTION *

How will the struggle for existence, briefly discussed in the last chapter, act in regard to variation? Can the principle of selection, which we have seen is so potent in the hands of man, apply under nature? I think we shall see that it can act most efficiently. Let the endless number of slight variations and individual differences occurring in our domestic productions, and, in a lesser degree, in those

* From the *Origin of Species*. Ch. IV.

under nature, be borne in mind ; as well as the strength of the hereditary tendency. Under domestication, it may be truly said that the whole organization becomes in some degree plastic. But the variability, which we almost universally meet with in our domestic production, is not directly produced, as Hooker and Asa Gray have well remarked, by man ; he can neither originate varieties, nor prevent their occurrence ; he can only preserve and accumulate such as do occur. Unintentionally he exposes organic beings to new and changing conditions of life, and variability ensues ; but similar changes of conditions might and do occur under nature. Let it also be borne in mind how infinitely complex and close-fitting are the mutual relations of all organic beings to each other and to their physical conditions of life ; and consequently what infinitely varied diversities of structure might be of use to each being under changing conditions of life. Can it then be thought improbable, seeing that variations useful to man have undoubtedly occurred, that other variations useful in some way to each being in the great and complex battle of life, should occur in the course of many successive generations ? If such do occur, can we doubt (remembering that many more individuals are born than can possibly survive) that individuals having any advantage, however slight, over others, would have the best chance of surviving and of procreating their kind ? On the other hand, we may feel sure that any variation in the least degree injurious would be rigidly destroyed. This preservation of favourable individual differences and variations, and the destruction of those which are injurious, I have called Natural Selection, or the Survival of the Fittest. Variations neither useful nor injurious would not be affected by natural selection, and would be left either a fluctuating element, as perhaps we see in certain polymorphic species, or would ultimately become fixed, owing to the nature of the organism and the nature of the conditions.

Several writers have misapprehended or objected to the term Natural Selection. Some have even imagined that natural selection induces variability, whereas it implies only the preservation of such variations as arise and are beneficial to the being under its conditions of life. No one objects to agriculturists speaking of the potent effects of man's selection ; and in this case the individual differences given by nature, which man for some object selects, must of necessity first

occur. Others have objected that the term selection implies conscious choice in the animals which become modified; and it has even been urged that, as plants have no volition, natural selection is not applicable to them! In the literal sense of the word, no doubt, natural selection is a false term; but who ever objected to chemists speaking of the elective affinities of the various elements?—and yet an acid cannot strictly be said to elect the base with which it in preference combines. It has been said that I speak of natural selection as an active power or Deity; but who objects to an author speaking of the attraction of gravity as ruling the movements of the planets? Everyone knows what is meant and is implied by such metaphorical expressions; and they are almost necessary for brevity. So again it is difficult to avoid personifying the word Nature; but I mean by Nature, only the aggregate action and product of many natural laws, and by laws the sequence of events as ascertained by us. With a little familiarity such superficial objections will be forgotten.

We shall best understand the probable course of natural selection by taking the case of a country undergoing some slight physical change, for instance, of climate. The proportional numbers of its inhabitants will almost immediately undergo a change, and some species will probably become extinct. We may conclude, from what we have seen of the intimate and complex manner in which the inhabitants of each country are bound together, that any change in the numerical proportions of the inhabitants, independently of the change of climate itself, would seriously affect the others. If the country were open on its borders, new forms would certainly immigrate, and this would likewise seriously disturb the relations of some of the former inhabitants. Let it be remembered how powerful the influence of a single introduced tree or mammal has been shown to be. But in the case of an island, or of a country partly surrounded by barriers, into which new and better adapted forms could not freely enter, we should then have places in the economy of nature which would assuredly be better filled up, if some of the original inhabitants were in some manner modified; for, had the area been open to immigration, these same places would have been seized on by intruders. In such cases, slight modifications, which in any way favoured the individuals of any species, by better adapting them to their altered

conditions, would tend to be preserved; and natural selection would have free scope for the work of improvement.

We have good reason to believe, as shown in the first chapter, that changes in the conditions of life give a tendency to increased variability; and in the foregoing cases the conditions have changed, and this would manifestly be favourable to natural selection, by affording a better chance of the occurrence of profitable variations. Unless such occur, natural selection can do nothing. Under the term of "variations," it must never be forgotten that mere individual differences are included. As man can produce a great result with his domestic animals and plants by adding up in any given direction individual differences, so could natural selection, but far more easily from having incomparably longer time for action. Nor do I believe that any great physical change, as of climate, or any unusual degree of isolation to check immigration, is necessary in order that new and unoccupied places should be left for natural selection to fill up by improving some of the varying inhabitants. For as all the inhabitants of each country are struggling together with nicely balanced forces, extremely slight modifications in the structure or habits of one species would often give it an advantage over others; and still further modifications of the same kind would often still further increase the advantage, as long as the species continued under the same conditions of life and profited by similar means of subsistence and defense. No country can be named in which all the native inhabitants are now so perfectly adapted to each other and to the physical conditions under which they live, that none of them could be still better adapted or improved; for in all countries, the natives have been so far conquered by naturalized productions, that they have allowed some foreigners to take firm possession of the land. And as foreigners have thus in every country beaten some of the natives, we may safely conclude that the natives might have been modified with advantage, so as to have better resisted the intruders.

As man can produce, and certainly has produced, a great result by his methodical and unconscious means of selection, what may not natural selection effect? Man can act only on external and visible characters: Nature, if I may be allowed to personify the natural preservation or survival of the fittest, cares nothing for appearances, except in so far as they are useful to any being. She can act on

every internal organ, on every shade of constitutional difference, on the whole machinery of life. Man selects only for his own good: Nature only for that of the being which she tends. Every selected character is fully exercised by her, as is implied by the fact of their selection. Man keeps the natives of many climates in the same country; he seldom exercises each selected character in some peculiar and fitting manner; he feeds a long and a short-beaked pigeon on the same food; he does not exercise a long-backed or long-legged quadruped in any peculiar manner; he exposes sheep with long and short wool to the same climate. He does not allow the most vigorous males to struggle for the females. He does not rigidly destroy all inferior animals, but protects during each varying season, as far as lies in his power, all his productions. He often begins his selection by some half-monstrous form; or at least by some modification prominent enough to catch the eye or to be plainly useful to him. Under nature, the slightest differences of structure or constitution may well turn the nicely-balanced scale in the struggle for life, and so be preserved. How fleeting are the wishes and efforts of man! how short his time! and consequently how poor will be his results, compared with those accumulated by Nature during whole geological periods! Can we wonder, then, that Nature's productions should be far "truer" in character than man's productions; that they should be infinitely better adapted to the most complex conditions of life, and should plainly bear the stamp of far higher workmanship?

It may metaphorically be said that natural selection is daily and hourly scrutinizing, throughout the world, the slightest variations; rejecting those that are bad, preserving and adding up all that are good; silently and sensibly working, whenever and wherever opportunity offers, at the improvement of each organic being in relation to its organic and inorganic conditions of life. We see nothing of these slow changes in progress, until the hand of time has marked the lapse of ages, and then so imperfect is our view into long-past geological ages, that we see only that the forms of life are now different from what they formerly were.

In order that any great amount of modification should be effected in a species, a variety when once formed must again, perhaps after a long interval of time, vary or present individual differences of the same favourable nature as before; and these must be again preserved,

and so onwards step by step. Seeing that individual differences of the same kind perpetually recur, this can hardly be considered as an unwarrantable assumption. But whether it is true, we can judge only by seeing how far the hypothesis accords with and explains the general phenomena of nature. On the other hand, the ordinary belief that the amount of possible variation is a strictly limited quantity is likewise a simple assumption.

Although natural selection can act only through and for the good of each being, yet characters and structures, which we are apt to consider as of very trifling importance, may thus be acted on. When we see leaf-eating insects green, and bark-feeders mottled gray; the Alpine ptarmigan white in winter, the red-grouse the colour of heather, we must believe that these tints are of service to these birds and insects in preserving them from danger. Grouse, if not destroyed at some period of their lives, would increase in countless numbers; they are known to suffer largely from birds of prey; and hawks are guided by eyesight to their prey—so much so, that on parts of the Continent persons are warned not to keep white pigeons, as being the most liable to destruction. Hence natural selection might be effective in giving the proper colour to each kind of grouse, and in keeping that colour, when once acquired, true and constant. Nor ought we to think that the occasional destruction of an animal of any particular colour would produce little effect: we should remember how essential it is in a flock of white sheep to destroy a lamb with the faintest trace of black. We have seen how the colour of the hogs, which feed on the “paint-root” in Virginia, determines whether they shall live or die. In plants, the down on the fruit and the colour of the flesh are considered by botanists as characters of the most trifling importance: yet we hear from an excellent horticulturist, Downing, that in the United States smooth-skinned fruits suffer far more from a beetle, a *Curculio*, than those with down; that purple plums suffer far more from a certain disease than yellow plums; whereas another disease attacks yellow-fleshed peaches far more than those with other coloured flesh. If, with all the aids of arts, these slight differences make a great difference in cultivating the several varieties, assuredly, in a state of nature, where the trees would have to struggle with other trees and with a host of enemies, such differences would effectually settle which variety, whether a smooth

or downy, a yellow or purple-fleshed fruit, should succeed.

In looking at many small points of difference between species, which, as far as our ignorance permits us to judge, seem quite unimportant, we must not forget that climate, food, etc., have no doubt produced some direct effect. It is also necessary to bear in mind that, owing to the law of correlation, when one part varies, and the variations are accumulated through natural selection, other modifications, often of the most unexpected nature, will ensue.

As we see that those variations which, under domestication, appear at any particular period of life, tend to reappear in the offspring at the same period; for instance, in the shape, size, and flavour of the seeds of the many varieties of our culinary and agricultural plants; in the caterpillar and cocoon stages of the varieties of the silkworm; in the eggs of poultry, and in the colour of the down of their chickens; in the horns of our sheep and cattle when nearly adult; so in a state of nature natural selection will be enabled to act on and modify organic beings at any age, by the accumulation of variations profitable at that age, and by their inheritance at a corresponding age. If it profit a plant to have its seeds more and more widely disseminated by the wind, I can see no greater difficulty in this being effected through natural selection, than in the cotton planter increasing and improving by selection the down in the pods on his cotton trees. Natural selection may modify and adapt the larva of an insect to a score of contingencies, wholly different from those which concern the mature insect; and these modifications may effect, through correlation, the structure of the adult. So, conversely, modifications in the adult may affect the structure of the larva; but in all cases natural selection will insure that they shall not be injurious: for if they were so, the species would become extinct.

Natural selection will modify the structure of the young in relation to the parent, and of the parent in relation to the young. In social animals it will adapt the structure of each individual for the benefit of the whole community; if the community profits by the selected change. What natural selection cannot do, is to modify the structure of one species; without giving it any advantage, for the good of another species; and though statements to this effect may be found in works of natural history, I cannot find one case which will bear investigation. A structure used only once in an animal's

life, if of high importance to it, might be modified to any extent by natural selection; for instance, the great jaws possessed by certain insects, used exclusively for opening the cocoon—or the hard tip of the beak of unhatched birds, used for breaking the egg. It has been asserted, that of the best short-beaked tumbler-pigeons a greater number perish in the egg than are able to get out of it; so that fanciers assist in the act of hatching. Now if nature had to make the beak of a full-grown pigeon very short for the bird's own advantage, the process of modification would be very slow, and there would be simultaneously the most rigorous selection of all the young birds within the egg, which had the most powerful and hardest beaks, for all with weak beaks would inevitably perish; or, more delicate and more easily broken shells might be selected, the thickness of the shell being known to vary like every other structure.

It may be well here to remark that with all beings there must be much fortuitous destruction, which can have little or no influence on the course of natural selection. For instance a vast number of eggs or seeds are annually devoured, and these could be modified through natural selection only if they varied in some manner which protected them from their enemies. Yet many of these eggs or seeds would perhaps, if not destroyed, have yielded individuals better adapted to their conditions of life than any of those which happened to survive. So again a vast number of mature animals and plants, whether or not they be the best adapted to their conditions, must be annually destroyed by accidental causes, which would not be in the least degree mitigated by certain changes of structure or constitution which would in other ways be beneficial to the species. But let the destruction of the adults be ever so heavy, if the number which can exist in any district be not wholly kept down by such causes,—or again let the destruction of eggs or seeds be so great that only a hundredth or a thousandth part are developed,—yet of those which do survive, the best adapted individuals, supposing that there is any variability in a favourable direction, will tend to propagate their kind in larger numbers than the less well adapted. If the numbers be wholly kept down by the causes just indicated, as will often have been the case, natural selection will be powerless in certain beneficial directions; but this is no valid objection to its efficiency at other times and in other ways; for we are far from having any reason to suppose that many species

ever undergo modification and improvement at the same time in the same area.

SEXUAL SELECTION

Inasmuch as peculiarities often appear under domestication in one sex and become hereditarily attached to that sex, so no doubt it will be under nature. Thus it is rendered possible for the two sexes to be modified through natural selection in relation to different habits of life, as is sometimes the case; or for one sex to be modified in relation to the other sex, as commonly occurs. This leads me to say a few words on what I have called Sexual Selection. This form of selection depends, not on a struggle for existence in relation to other organic beings or to external conditions, but on a struggle between the individuals of one sex, generally the males, for the possession of the other sex. The result is not death to the unsuccessful competitor, but few or no offspring. Sexual selection is, therefore, less rigorous than natural selection. Generally, the most vigorous males, those which are best fitted for their places in nature, will leave most progeny. But in many cases, victory depends not so much on general vigour, as on having special weapons, confined to the male sex. A hornless stag or spurless cock would have a poor chance of leaving numerous offspring. Sexual selection, by always allowing the victor to breed, might surely give indomitable courage, length to the spur, and strength to the wing to strike in the spurred leg, in nearly the same manner as does the brutal cockfighter by the careful selection of his best cocks. How low in the scale of nature the law of battle descends, I know not; male alligators have been described as fighting, bellowing, and whirling round, like Indians in a war-dance, for the possession of the females; male salmons have been observed fighting all day long; male stag-beetles sometimes bear wounds from the huge mandibles of other males; the males of certain hymenopterous insects have been frequently seen by that inimitable observer, M. Fabre, fighting for a particular female who sits by, an apparently unconcerned beholder of the struggle, and then retires with the conquerer. The war is, perhaps, severest between the males of polygamous animals, and these seem oftenest provided with special weapons. The males of carnivorous animals are already well armed; though to them and to others, special means of defence may be given through means of

sexual selection, as the mane of the lion, and the hooked jaw to the male salmon; for the shield may be as important for victory as the sword or spear.

Amongst birds, the contest is often of a more peaceful character. All those who have attended to the subject believe that there is the severest rivalry between the males of many species to attract, by singing, the females. The rock-thrush of Guiana, birds of paradise, and some others, congregate; and successive males display with the most elaborate care, and show off in the best manner, their gorgeous plumage; they likewise perform strange antics before the females, which, standing by as spectators, at last choose the most attractive partner. Those who have closely attended to birds in confinement well know that they often take individual preferences and dislikes: thus Sir R. Heron has described how a pied peacock was eminently attractive to all his hen birds. I cannot here enter on the necessary details; but if man can in a short time give beauty and an elegant carriage to his bantams, according to his standard of beauty, I can see no good reason to doubt that female birds, by selecting, during thousands of generations, the most melodious or beautiful males, according to their standard of beauty, might produce a marked effect. Some well-known laws, with respect to the plumage of male and female birds, in comparison with the plumage of the young, can partly be explained through the action of sexual selection on variations occurring at different ages, and transmitted to the males alone or to both sexes at corresponding ages; but I have not space here to enter on this subject.

Thus it is, as I believe, that when the males and females of any animal have the same general habits of life, but differ in structure, colour, or ornament, such differences have been mainly caused by sexual selection: that is, by individual males having had, in successive generations, some slight advantage over other males, in their weapons, means of defence, or charms, which they have transmitted to their male offspring alone. Yet, I would not wish to attribute all sexual differences to this agency: for we see in our domestic animals peculiarities arising and becoming attached to the male sex, which apparently have not been augmented through selection by man. The tuft of hair on the breast of the wild turkey-cock cannot be of any use, and it is doubtful whether it can be ornamental in the eyes of the female

bird;—indeed, had the tuft appeared under domestication, it would have been called a monstrosity.

ON THE DEGREE TO WHICH ORGANISATION TENDS TO ADVANCE

Natural Selection acts exclusively by the preservation and accumulation of variations, which are beneficial under the organic and inorganic conditions to which each nature is exposed at all periods of life. The ultimate result is that each creature tends to become more and more improved in relation to its conditions. This improvement inevitably leads to the gradual advancement of the organisation of the greater number of living beings throughout the world. But here we enter on a very intricate subject, for naturalists have not defined to each other's satisfaction what is meant by an advance in organisation. Amongst the vertebrata the degree of intellect and an approach in structure to man clearly come into play. It might be thought that the amount of change which the various parts and organs pass through in their development from the embryo to maturity would suffice as a standard of comparison; but there are cases, as with certain parasitic crustaceans, in which several parts of the structure become less perfect, so that the mature animal cannot be called higher than its larva. Von Bar's standard seems the most widely applicable and the best, namely, the amount of differentiation of the parts of the same organic being, in the adult state as I should be inclined to add, and their specialisation for different functions; or, as Milne Edwards would express it, the completeness of the division of physiological labour. But we shall see how obscure this subject is if we look, for instance, to fishes, amongst which some naturalists rank those as highest which, like the sharks, approach nearest to amphibians; whilst other naturalists rank the common bony or teleostean fishes as the highest, inasmuch as they are most strictly fishlike, and differ most from the other vertebrate classes. We see still more plainly the obscurity of the subject by turning to plants, amongst which the standard of intellect is of course quite excluded; and here some botanists rank those plants as highest which have every organ, as sepals, petals, stamens, and pistils, fully developed in each flower; whereas other botanists, probably with more truth, look at the plants which have their several organs much modified and reduced in number as the highest.

If we take as the standard of high organisation, the amount of differentiation and specialisation of the several organs in each being when adult (and this will include the advancement of the brain for intellectual purposes), natural selection clearly leads towards this standard; for all physiologists admit that the specialisation of organs, inasmuch as in this state they perform their functions better, is an advantage to each being; and hence the accumulation of variations tending towards specialisation is within the scope of natural selection. On the other hand, we can see, bearing in mind that all organic beings are striving to increase at a high ratio and to seize on every unoccupied or less well occupied place in the economy of nature, that it is quite possible for natural selection gradually to fit a being to a situation in which several organs would be superfluous or useless: in such cases there would be retrogression in the scale of organisation. Whether organisation on the whole has actually advanced from the remotest geological periods to the present day will be more conveniently discussed in our chapter on Geological Succession.

But it may be objected that if all organic beings thus tend to rise in the scale, how is it that throughout the world a multitude of the lowest forms still exist; and how is it that in each great class some forms are far more highly developed than others? Why have not the more highly developed forms everywhere supplanted and exterminated the lower? Lamarck, who believed in an innate and inevitable tendency towards perfection in all organic beings, seems to have felt this difficulty so strongly, that he was led to suppose that new and simple forms are continually being produced by spontaneous generation. Science has not as yet proved the truth of this belief, whatever the future may reveal. On our theory the continued existence of lowly organisms offers no difficulty; for natural selection, or the survival of the fittest, does not necessarily include progressive development—it only takes advantage of such variations as arise and are beneficial to each creature under its complex relations of life. And it may be asked what advantage, as far as we can see, would it be to an infusorian animalcule—to an intestinal worm—or even to an earthworm, to be highly organised. If it were no advantage, these forms would be left, by natural selection, unimproved or but little improved, and might remain for indefinite ages in their present lowly condition. And geology tells us that some of the lowest forms, as the infusoria

and rhizopods, have remained for an enormous period in nearly their present state. But to suppose that most of the many now existing low forms have not in the least advanced since the first dawn of life would be extremely rash; for every naturalist who has dissected some of the beings now ranked as very low in the scale, must have been struck with their really wondrous and beautiful organisation.

Nearly the same remarks are applicable if we look to the different grades of organisation within the same great group; for instance, in the vertebrata, to the co-existence of mammals and fish—amongst mammalia, to the co-existence of man and the ornithorhynchus—amongst fishes, to the co-existence of the shark and the lancelet (*Amphioxus*), which latter fish in the extreme simplicity of its structure approaches the invertebrate classes. But mammals and fish hardly come into competition with each other; the advancement of the whole class of mammals, or of certain members in this class, to the highest grade would not lead to their taking the place of fishes. Physiologists believe that the brain must be bathed by warm blood to be highly active, and this requires aërial respiration; so that warm-blooded mammals when inhabiting the water lie under a disadvantage in having to come continually to the surface to breathe. With fishes, members of the shark family would not tend to supplant the lancelet; for the lancelet, as I hear from Fritz Müller, has as sole companion and competitor on the barren, sandy shore of South Brazil, an anomalous annelid. The three lowest orders of mammals, namely, marsupials, edentata, and rodents, co-exist in South America in the same region with numerous monkeys, and probably interfere little with each other. Although organisation, on the whole, may have advanced and be still advancing throughout the world, yet the scale will always present many degrees of perfection; for the high advancement of certain whole classes, or of certain members of each class, does not at all necessarily lead to the extinction of those groups with which they do not enter into close competition. In some cases, as we shall hereafter see, lowly organised forms appear to have been preserved to the present day, from inhabiting confined or peculiar stations, where they have been subjected to less severe competition, and where their scanty numbers have retarded the chance of favourable variations arising.

Finally, I believe that many lowly organised forms now exist

throughout the world, from various causes. In some cases variations or individual differences of a favourable nature may never have arisen for natural selection to act on and accumulate. In no case, probably, has time sufficed for the utmost possible amount of development. In some few cases there has been what we must call retrogression of organisation. But the main cause lies in the fact that under very simple conditions of life a high organisation would be of no service,—possibly would be of actual disservice, as being of a more delicate nature, and more liable to be put out of order and injured.

Looking to the first dawn of life, when all organic beings, as we may believe, presented the simplest structure, how, it has been asked, could the first steps in the advancement of differentiation of parts have arisen? Mr. Herbert Spencer would probably answer that, as soon as simple unicellular organism came by growth or division to be compounded of several cells, or became attached to any supporting surface, his law “that homologous units of any order become differentiated in proportion as their relations to incident forces become different” would come into action. But as we have no facts to guide us, speculation on the subject is almost useless. It is, however, an error to suppose that there would be no struggle for existence, and, consequently, no natural selection, until many forms had been produced; variations in a single species inhabiting an isolated station might be beneficial, and thus the whole mass of individuals might be modified, or two distinct forms might arise. But, as I remarked towards the close of the Introduction, no one ought to feel surprise at much remaining as yet unexplained on the origin of species, if we make due allowance for our profound ignorance on the mutual relations of the inhabitants of the world at the present time, and still more so during past ages.

CONVERGENCE OF CHARACTER

Mr. H. C. Watson thinks that I have overrated the importance of divergence of character (in which, however, he apparently believes), and that convergence, as it may be called, has likewise played a part. If two species, belonging to two distinct though allied genera, had both produced a large number of new and divergent forms, it is conceivable that these might approach each other so closely that they would have all to be classed under the same genus; and thus the

descendants of two distinct genera would converge into one. But it would in most cases be extremely rash to attribute to convergence a close and general similarity of structure in the modified descendants of widely distinct forms. The shape of a crystal is determined solely by the molecular forces, and it is not surprising that dissimilar substances should sometimes assume the same form; but with organic beings we should bear in mind that the form of each depends on an infinitude of complex relations, namely, on the variations which have arisen, those being due to causes far too intricate to be followed out,—on the nature of the variations which have been preserved or selected, and this depends on the surrounding physical conditions, and in a still higher degree on the surrounding organisms with which each being has come into competition,—and lastly, on inheritance (in itself a fluctuating element) from innumerable progenitors, all of which have had their forms determined through equally complex relations. It is incredible that the descendants of two organisms, which had originally differed in a marked manner, should ever afterwards converge so closely as to lead to a near approach to identity throughout their whole organisation. If this had occurred, we should meet with the same form, independently of genetic connection, recurring in widely separated geological formations; and the balance of evidence is opposed to any such an admission.

Mr. Watson has also objected that the continued action of natural selection, together with divergence of character, would tend to make an indefinite number of specific forms. As far as mere inorganic conditions are concerned, it seems probable that a sufficient number of species would soon become adapted to all considerable diversities of heat, moisture, &c.; but I fully admit that the mutual relations of organic beings are more important; and as the number of species in any country goes on increasing, the organic conditions of life must become more and more complex. Consequently there seems at first sight no limit to the amount of profitable diversification of structure, and therefore no limit to the number of species which might be produced. We do not know that even the most prolific area is fully stocked with specific forms: at the Cape of Good Hope and in Australia, which support such an astonishing number of species, many European plants have become naturalised. But geology shows

us, that from an early part of the tertiary period the number of species of shells, and that from the middle part of this same period the number of mammals, has not greatly or at all increased. What then checks an indefinite increase in the number of species? The amount of life (I do not mean the number of specific forms) supported on an area must have a limit, depending so largely as it does on physical conditions; therefore, if an area be inhabited by very many species, each or nearly each species will be represented by few individuals; and such species will be liable to exterminate from accidental fluctuations in the nature of the seasons or in the number of their enemies. The process of extermination in such cases would be rapid, whereas the production of new species must always be slow. Imagine the extreme case of as many species as individuals in England, and the first severe winter or very dry summer would exterminate thousands on thousands of species. Rare species, and each species will become rare if the number of species in any country becomes indefinitely increased, will, on the principle often explained, present within a given period few favourable variations; consequently, the process of giving birth to new specific forms would thus be retarded. When any species becomes very rare, close interbreeding will help to exterminate it; authors have thought that this comes into play in accounting for the deterioration of the Aurochs in Lithuania, of Red Deer in Scotland, and of Bears in Norway, &c. Lastly, and this I am inclined to think is the most important element, a dominant species, which has already beaten many competitors in its own home, will tend to spread and supplant many others. Alph. de Candolle has shown that those species which spread widely, tend generally to spread very widely; consequently, they will tend to supplant and exterminate several species in several areas, and thus check the inordinate increase of specific forms throughout the world. Dr. Hooker has recently shown that in the S. E. corner of Australia, where, apparently, there are many invaders from different quarters of the globe, the endemic Australian species have been greatly reduced in number. How much weight to attribute to these several considerations I will not pretend to say; but conjointly they must limit in each country the tendency to an indefinite augmentation of specific forms.

SUMMARY OF CHAPTER

If under changing conditions of life organic beings present individual differences in almost every part of their structure, and this cannot be disputed; if there be, owing to their geometrical rate of increase, a severe struggle for life at some age, season, or year, and this certainly cannot be disputed; then, considering the infinite complexity of the relations of all organic beings to each other and to their conditions of life, causing an infinite diversity in structure, constitution, and habits, to be advantageous to them, it would be a most extraordinary fact if no variations had ever occurred useful to each being's own welfare, in the same manner as so many variations have occurred useful to man. But if variations useful to any organic being ever do occur, assuredly individuals thus characterised will have the best chance of being preserved in the struggle for life; and from the strong principle of inheritance, these will tend to produce offspring similarly characterised. This principle of preservation, or the survival of the fittest, I have called Natural Selection. It leads to the improvement of each creature in relation to its organic and inorganic conditions of life; and consequently, in most cases, to what must be regarded as an advance in organisation. Nevertheless, low and simple forms will long endure if well fitted for their simple conditions of life.

Natural selection, on the principle of qualities being inherited at corresponding ages, can modify the egg, seed, or young, as easily as the adult. Amongst many animals, sexual selection will have given its aid to ordinary selection, by assuring to the most vigorous and best adapted males the greatest number of offspring. Sexual selection will also give characters useful to the males alone, in their struggles or rivalry with other males; and these characters will be transmitted to one sex or to both sexes, according to the form of inheritance which prevails.

Whether natural selection has really thus acted in adapting the various forms of life to their several conditions and stations, must be judged by the general tenor and balance of evidence given in the following chapters. But we have already seen how it entails extinction; and how largely extinction has acted in the world's history, geology plainly declares. Natural selection, also, leads to divergence of char-

acter; for the more organic beings diverge in structure, habits, and constitution, by so much the more can a large number be supported on the area,—of which we see proof by looking to the inhabitants of any small spot, and to the productions naturalised in foreign lands. Therefore, during the modification of the descendants of any one species, and during the incessant struggle of all species to increase in numbers, the more diversified the descendants become, the better will be their chance of success in the battle for life. Thus the small differences distinguishing varieties of the same species, steadily tend to increase, till they equal the greater differences between species of the same genus, or even of distinct genera.

We have seen that it is the common, the widely diffused and widely ranging species, belonging to the larger genera within each class, which vary most; and these tend to transmit to their modified offspring that superiority which now makes them dominant in their own countries. Natural selection, as has just been remarked, leads to divergence of character and to much extinction of the less improved and intermediate forms of life. On these principles, the nature of the affinities, and the generally well-defined distinctions between the innumerable organic beings in each class throughout the world, may be explained. It is a truly wonderful fact—the wonder of which we are apt to overlook from familiarity—that all animals and all plants throughout all time and space should be related to each other in groups, subordinate to groups, in the manner which we everywhere behold—namely, varieties of the same species most closely related, species of the same genus less closely and unequally related, forming sections and sub-genera, species of distinct genera much less closely related, and genera related in different degrees, forming sub-families, families, orders, sub-classes and classes. The several subordinate groups in any class cannot be ranked in a single file, but seem clustered round points, and these round other points, and so on in almost endless cycles. If species had been independently created, no explanation would have been possible of this kind of classification; but it is explained through inheritance and the complex action of natural selection, entailing extinction and divergence of character . . .

The affinities of all the beings of the same class have sometimes been represented by a great tree. I believe this simile largely speaks

the truth. The green and budding twigs may represent existing species; and those produced during former years may represent the long succession of extinct species. At each period of growth all the growing twigs have tried to branch out on all sides, and to overtop and kill the surrounding twigs and branches, in the same manner as species and groups of species have at all times overmastered other species in the great battle for life. The limbs divided into great branches, and these into lesser and lesser branches, were themselves once, when the tree was young, budding twigs; and this connection of the former and present buds by ramifying branches may well represent the classification of all extinct and living species in groups subordinate to groups. Of the many twigs which flourished when the tree was a mere bush, only two or three, now grown into great branches, yet survive and bear the other branches; so with the species which lived during long-past geological periods, very few have left living and modified descendants. From the first growth of the tree, many a limb and branch has decayed and dropped off; and these fallen branches of various sizes may represent those whole orders, families, and genera which have now no living representatives, and which are known to us only in a fossil state. As we here and there see a thin straggling branch springing from a fork low down in a tree, and which by some chance has been favoured and is still alive on its summit, so we occasionally see an animal like the *Ornithorhynchus* or *Lepidosiren*, which in some small degree connects by its affinities two large branches of life, and which has apparently been saved from fatal competition by having inhabited a protected station. As buds give rise by growth to fresh buds, and these, if vigorous, branch out and overtop on all sides many a feeble branch, so by generation I believe it has been with the great Tree of Life, which fills with its dead and broken branches the crust of the earth, and covers the surface with its ever-branching and beautiful ramifications.

XXX

THEODOR SCHWANN

1810-1882

Theodor Schwann, the son of a Prussian printer, was born at Neuss, Prussia, December 7, 1810. He first studied medicine, but was persuaded to devote himself to science by Johannes Mueller, who appointed him assistant in the anatomical museum. In 1838 he was called to the Catholic University of Louvain, and later removed to Liège. One of the first to suggest the chemical explanation of life, he discovered the presence and function of pepsin as a ferment in digestion. In 1839 he established his great theory that all life is composed of inter-connected cellular units—a conception which revolutionized biology. He died at Liège on January 11, 1882.

CELL THEORY *

The various opinions entertained with respect to the fundamental powers of an organized body may be reduced to two, which are essentially different from one another. The first is, that every organism originates with an inherent power, which models it into conformity with a predominant idea, arranging the molecules in the relation necessary for accomplishing certain purposes held forth by this idea. Here, therefore, that which arranges and combines the molecules is a power acting with a definite purpose. A power of this kind would be essentially different from all the powers of inorganic nature, because action goes on in the latter quite blindly. A certain impression is followed of necessity by a certain change of quality and quantity,

* Translated from *Mikroskopische Untersuchungen über die Wachstum der Tiere und der Pflanzen* (Berlin, 1839) by Henry Smith in the *Publications of the Sydenham Society* (1847).

without regard to any purpose. In this view, however, the fundamental power of the organism (or the soul, in the sense employed by Stahl) would, inasmuch as it works with a definite individual purpose, be much more nearly allied to the immaterial principle, endued with consciousness which we must admit operates in man.

The other view is, that the fundamental powers of organized bodies agree essentially with those of inorganic nature, that they work altogether blindly according to laws of necessity and irrespective of any purpose, that they are powers which are as much established with the existence of matter as the physical powers are. It might be assumed that the powers which form organized bodies do not appear at all in inorganic nature, because this or that particular combination of molecules, by which the powers are elicited, does not occur in inorganic nature, and yet they might not be essentially distinct from physical and chemical powers. It cannot, indeed, be denied that adaptation to a particular purpose, in some individuals even in a high degree, is characteristic of every organism; but, according to this view, the source of this adaptation does not depend upon each organism being developed by the operation of its own power in obedience to that purpose, but it originates as in inorganic nature, in the creation of the matter with its blind powers by a rational Being. We know, for instance, the powers which operate in our planetary system. They operate, like all physical powers, in accordance with blind laws of necessity, and yet is the planetary system remarkable for its adaptation to a purpose. The ground of this adaptation does not lie in the powers, but in Him, who has so constituted matter with its powers, that in blindly obeying its laws it produces a whole suited to fulfil an intended purpose. We may even assume that the planetary system has an individual adaptation to a purpose. Some external influence, such as a comet, may occasion disturbances of motion, without thereby bringing the whole into collision; derangements may occur on single planets, such as a high tide, &c., which are yet balanced entirely by physical laws. As respects their adaptation to a purpose, organized bodies differ from these in degree only; and by this second view we are just as little compelled to conclude that the fundamental powers of organization operate according to laws of adaptation to a purpose, as we are in inorganic nature.

The first view of the fundamental powers of organized bodies may

be called the teleological, the second the physical view. An example will show at once, how important for physiology is the solution of the question as to which is to be followed. If, for instance, we define inflammation and suppuration to be the effort of the organism to remove a foreign body that has been introduced into it; or fever to be the effort of the organism to eliminate diseased matter, and both as the result of the "autocracy of the organism," then these explanations accord with the teleological view. For, since by these processes the obnoxious matter is actually removed, the process which effects them is one adapted to an end; and as the fundamental power of the organism operates in accordance with definite purposes, it may either set these processes in action primarily, or may also summon further powers of matter to its aid, always, however, remaining itself the "primum movens." On the other hand, according to the physical view, this is just as little an explanation as it would be to say, that the motion of the earth around the sun is an effort of the fundamental power of the planetary system to produce a change of seasons on the planets, or to say, that ebb and flood are the reaction of the organism of the earth upon the moon.

In physics, all those explanations which were suggested by a teleological view of nature, as "horror vacui," and the like, have long been discarded. But in animated nature, adaptation—individual adaptation—to a purpose is so prominently marked, that it is difficult to reject all teleological explanations. Meanwhile it must be remembered that those explanations, which explain at once all and nothing, can be but the last resources, when no other view can possibly be adopted; and there is no such necessity for admitting the teleological view in the case of organized bodies. The adaptation of a purpose which is characteristic of organized bodies differs only in degree from what is apparent also in the inorganic part of nature; and the explanation that organized bodies are developed, like all the phenomena of inorganic nature, by the operation of blind laws framed with the matter, cannot be rejected as impossible. Reason certainly requires some ground for such adaptation, but for her it is sufficient to assume that matter with the powers inherent in it owes its existence to a rational Being. Once established and preserved in their integrity, these powers may, in accordance with their immutable laws of blind necessity, very well produce combinations, which manifest, even in a high

degree, individual adaptation to a purpose. If, however, rational power interpose after creation merely to sustain, and not as an immediately active agent, it may, so far as natural science is concerned, be entirely excluded from the consideration of the creation.

But the teleological view leads to further difficulties in the explanation, and especially with respect to generation. If we assume each organism to be formed by a power which acts according to a certain predominant idea, a portion of this power may certainly reside in the ovum during generation; but then we must ascribe to this subdivision of the original power, at the separation of the ovum from the body of the mother, the capability of producing an organism similar to that which the power, of which it is but a portion, produced: that is, we must assume that this power is infinitely divisible, and yet that each part may perform the same actions as the whole power. If, on the other hand, the power of organized bodies reside, like the physical powers, in matter as such, and be set free only by a certain combination of the molecules, as, for instance, electricity is set free by the combination of a zinc and copper plate, then also by the conjunction of molecules to form an ovum the power may be set free, by which the ovum is capable of appropriating to itself fresh molecules, and these newly-conjoined molecules again by this very mode of combination acquire the same power to assimilate fresh molecules. The first development of the many forms of organized bodies—the progressive formation of organic nature indicated by geology—is also much more difficult to understand according to the teleological than the physical view.

Another objection to the teleological view may be drawn from the foregoing investigation. The molecules, as we have seen, are not immediately combined in various ways, as the purpose of the organism requires, but the formation of the elementary parts of organic bodies is regulated by laws which are essentially the same for all elementary parts. One can see no reason why this should be the case, if each organism be endued with a special power to frame the parts according to the purpose which they have to fulfil: it might much rather be expected that the formative principle, although identical for organs physiologically the same, would yet in different tissues be correspondingly varied. This resemblance of the elementary parts has, in the

instance of plants, already led to the conjecture that the cells are really the organisms, and that the whole plant is an aggregate of these organisms arranged according to certain laws. But since the elementary parts of animals bear exactly similar relations, the individuality of an entire animal would thus be lost; and yet precisely upon the individuality of the whole animal does the assumption rest, that it possesses a single fundamental power operating in accordance with a definite idea.

Meanwhile, we cannot altogether lay aside teleological views if all phenomena are not clearly explicable by the physical view. It is, however, unnecessary to do so, because an explanation, according to the teleological view, is only admissible when the physical can be shown to be impossible. In any case it conduces much more to the object of science to strive, at least, to adopt the physical explanation. And I would repeat that, when speaking of a physical explanation of organic phenomena, it is not necessary to understand an explanation by known physical powers, such, for instance, as that universal refuge electricity, and the like; but an explanation by means of powers which operate like the physical powers, in accordance with strict laws of blind necessity, whether they be also to be found in inorganic nature or not.

We set out, therefore, with the supposition that an organized body is not produced by a fundamental power which is guided in its operation by a definite idea, but is developed, according to blind laws of necessity, by powers which, like those of inorganic nature, are established by the very existence of matter. As the elementary materials of organic nature are not different from those of the inorganic kingdom, the source of the organic phenomena can only reside in another combination of these materials, whether it be in a peculiar mode of union of the elementary atoms to form atoms of the second order, or in the arrangement of these conglomerate molecules when forming either the separate morphological elementary parts of organisms, or an entire organism. We have here to do with the latter question solely, whether the cause of organic phenomena lies in the whole organism, or in its separate elementary parts. If this question can be answered, a further inquiry still remains as to whether the organism or its elementary parts possess this power through the

peculiar mode of combination of the conglomerate molecules, or through the mode in which the elementary atoms are united into conglomerate molecules.

We may, then, form the two following ideas of the cause of organic phenomena, such as growth, &c. First, that the cause resides in the totality of the organism. By the combination of the molecules into a systematic whole, such as the organism is in every stage of its development, a power is engendered, which enables such an organism to take up fresh material from without, and appropriate it either to the formation of new elementary parts, or to the growth of those already present. Here, therefore, the cause of the growth of the elementary parts resides in the totality of the organism. The other mode of explanation is, that growth does not ensue from a power resident in the entire organism, but that each separate elementary part is possessed of an independent power, an independent life, so to speak; in other words, the molecules in each separate elementary part are so combined as to set free a power by which it is capable of attracting new molecules, and so increasing, and the whole organism subsists only by means of the reciprocal action of the single elementary parts. So that here the single elementary parts only exert an active influence on nutrition, and totality of the organism may indeed be a condition, but is not in this view a cause.

In order to determine which of these two views is the correct one, we must summon to our aid the results of the previous investigation. We have seen that all organized bodies are composed of essentially similar parts, namely, of cells; that these cells are formed and grow in accordance with essentially similar laws; and, therefore, that these processes must, in every instance, be produced by the same powers. Now, if we find that some of these elementary parts, not differing from the others, are capable of separating themselves from the organism, and pursuing an independent growth, we may thence conclude that each of the other elementary parts, each cell, is already possessed of power to take up fresh molecules and growth; and that, therefore, every elementary part possesses a power of its own, an independent life, by means of which it would be enabled to develop itself independently, if the relations which it bore to external parts were but similar to those in which it stands in the organism. The ova of animals afford us example of such independent cells, growing apart from the

organism. It may, indeed, be said of the ova of higher animals, that after impregnation the ovum is essentially different from the other cells of the organism; that by impregnation there is a something conveyed to the ovum, which is more to it than an external condition for vitality, more than nutrient matter; and that it might thereby have first received its peculiar vitality, and therefore that nothing can be inferred from it with respect to the other cells. But this fails in application to those classes which consist only of female individuals, as well as with the spores of the lower plants; and, besides, in the inferior plants any given cell may be separated from the plant, and then grow alone. So that here are whole plants consisting of cells, which can be positively proved to have independent vitality. Now, as all cells grow according to the same laws, and consequently the cause of growth cannot in one case lie in the cell, and in another in the whole organism; and since it may be further proved that some cells, which do not differ from the rest in their mode of growth, are developed independently, we must ascribe to all cells an independent vitality, that is, such combinations of molecules as occur in any single cell, are capable of setting free the power by which it is enabled to take up fresh molecules. The cause of nutrition and growth resides not in the organism as a whole, but in the separate elementary parts—the cells. The failure of growth in the case of any particular cell, when separated from an organized body, is as slight an objection to this theory as it is an objection against the independent vitality of a bee, that it cannot continue long in existence after being separated from its swarm. The manifestation of the power which resides in the cell depends upon conditions to which it is subject only when in connexion with the whole (organism).

The question, then, as to the fundamental power of organized bodies resolves itself into that of the fundamental powers of the individual cells. We must now consider the general phenomena attending the formation of cells, in order to discover what powers may be presumed to exist in the cells to explain them. These phenomena may be arranged in two natural groups: first, those which relate to the combination of the molecules to form a cell, and which may be denominated the plastic phenomena of the cells; secondly, those which result from chemical changes either in the component particles of the cell itself, or in the surrounding cytotblastema, and which may be called meta-

bolic phenomena (*to metabolikon*, implying that which is liable to occasion or to suffer change).

The general plastic appearances in the cells are, as we have seen, the following: at first a minute corpuscle is formed (the nucleolus); a layer of substance (the nucleus) is then precipitated around it, which becomes more thickened and expanded by the continual deposition of fresh molecules between those already present. Deposition goes on more vigorously at the outer part of this layer than at the inner. Frequently the entire layer, or in other instances the outer part of it only, becomes condensed to a membrane, which may continue to take up new molecules in such a manner that it increases more rapidly in superficial extent than in thickness, and thus an intervening cavity is necessarily formed between it and the nucleolus. A second layer (cell) is next precipitated around this first, in which precisely the same phenomena are repeated, with merely the difference that in this case the processes, especially the growth of the layer and the formation of the space intervening between it and the first layer (the cell-cavity), go on more rapidly and more completely. Such were the phenomena in the formation of most cells; in some, however, there appeared to be only a single layer formed, while in others (those especially in which the nucleolus was hollow) there were three. The other varieties in the development of the elementary parts were (as we saw) reduced to these—that if two neighbouring cells commence their formation so near to one another that the boundaries of the layers forming around each of them meet at any spot, a common layer may be formed enclosing the two incipient cells. So at least the origin of nuclei, with two or more nucleoli, seemed explicable, by a coalescence of the first layers (corresponding to the nucleus), and the union of many primary cells into one secondary cell by a similar coalescence of the second layers (which correspond to the cell). But the further development of these common layers proceeds as though they were only an ordinary single layer. Lastly, there were some varieties in the progressive development of the cells, which were referable to an unequal deposition of the new molecules between those already present in the separate layers. In this way modifications of form and division of the cells were explained. And among the number of the plastic phenomena in the cells we may mention, lastly, the formation of secondary deposits; for instances occur in which one or

more new layers, each on the inner surface of the previous one, are deposited on the inner surface of a simple or of a secondary cell.

These are the most important phenomena observed in the formation and development of cells. The unknown cause, presumed to be capable of explaining these processes in the cells, may be called the plastic power of the cells. We will, in the next place, proceed to determine how far a more accurate definition of this power may be deduced from these phenomena.

In the first place, there is a power of attraction exerted in the very commencement of the cell, in the nucleolus, which occasions the addition of new molecules to those already present. We may imagine the nucleolus itself to be first formed by a sort of crystallization from out of a concentrated fluid. For if a fluid be so concentrated that the molecules of the substance in solution exert a more powerful mutual attraction than is exerted between them and the molecules of the fluid in which they are dissolved, a part of the solid substance must be precipitated. One can readily understand that the fluid must be more concentrated when new cells are being formed in it than when those already present have merely to grow. For if the cell is already partly formed, it exerts an attractive force upon the substance still in solution. There is then a cause for the deposition of this substance, which does not co-operate when no part of the cell is yet formed. Therefore, the greater the attractive force of the cell is, the less concentration of the fluid is required; while, at the commencement of the formation of a cell, the fluid must be more than concentrated. But the conclusion which may be thus directly drawn, as to the attractive power of the cell, may also be verified by observation. Wherever the nutrient fluid is not equally distributed in a tissue, the new cells are formed in that part into which the fluid penetrates first, and where, consequently, it is most concentrated. Upon this fact, as we have seen, depended the difference between the growth of organized and unorganized tissues. And this confirmation of the foregoing conclusion by experience speaks also for the correctness of the reasoning itself.

The attractive power of the cells operates so as to effect the addition of new molecules in two ways,—first, in layers, and secondly, in such a manner in each layer that the new molecules are deposited between those already present. This is only an expression of the fact; the

more simple law, by which several layers are formed and the molecules are not all deposited between those already present, cannot yet be explained. The formation of layers may be repeated once, twice, or thrice. The growth of the separate layers is regulated by a law, that the deposition of new molecules should be greatest at the part where the nutrient fluid is most concentrated. Hence the outer part particularly becomes condensed into a membrane both in the layer corresponding to the nucleus and in that answering to the cell, because the nutrient fluid penetrates from without, and consequently is more concentrated at the outer than at the inner part of each layer. For the same reason the nucleus grows rapidly, so long as the layer of the cell is not formed around it, but it either stops growing altogether, or at least grows much more slowly as soon as the cell-layer has surrounded it; because then the latter receives the nutrient matter first, and, therefore, in a more concentrated form. And hence the cell becomes, in a general sense, much more completely developed, while the nucleus-layer usually remains at a stage of development, in which the cell-layer had been in its earlier period. The addition of new molecules is so arranged that the layers increase more considerably in superficial extent than in thickness; and thus an intervening space is formed between each layer and the one preceding it, by which cells and nuclei are formed into actual hollow vesicles. From this it may be inferred that the deposition of new molecules is more active between those which lie side by side along the surface of the membrane, than between those which lie one upon the other in its thickness. Were it otherwise, each layer would increase in thickness, but there would be no intervening cavity between it and the previous one, there would be no vesicles, but a solid body composed of layers.

Attractive power is exerted in all the solid parts of the cell. This follows, not only from the fact that new molecules may be deposited everywhere between those already present, but also from the formation of secondary deposits. When the cavity of a cell is once formed, material may be also attracted from its contents and deposited in layers; and as this deposition takes place upon the inner surface of the membrane of the cell, it is probably that which exerts the attractive influence. This formation of layers on the inner surface of the cell-membrane is, perhaps, merely a repetition of the same process by

which, at an earlier period, nucleus and cell were precipitated as layers around the nucleolus. It must, however, be remarked that the identity of these two processes cannot be so clearly proved as that of the processes by which nucleus and cell are formed; more especially as there is a variety in the phenomena, for the secondary deposits in plants occur in spiral forms, while this has at least not yet been demonstrated in the formation of the cell-membrane and the nucleus, although by some botanical writers the cell-membrane itself is supposed to consist of spirals.

The power of attraction may be uniform throughout the whole cell, but it may also be confined to single spots; the deposition of new molecules is then more vigorous at these spots, and the consequence of this uneven growth of the cell-membrane is a change in the form of the cell.

The attractive power of the cells manifest a certain form of election in its operation. It does not take up all the substances contained in the surrounding cytotblastema, but only particular ones, either those which are analogous with the substance already present in the cell (assimilation), or such as differ from it in chemical properties. The several layers grow by assimilation, but when a new layer is being formed, different material from that of the previously-formed layer is attracted: for the nucleolus, the nucleus and cell-membrane are composed of materials which differ in their chemical properties.

Such are the peculiarities of the plastic power of the cells, so far as they can as yet be drawn from observation. But the manifestations of this power presuppose another faculty of the cells. The cytotblastema, in which the cells are formed, contains the elements of the materials of which the cell is composed, but in other combinations; it is not a mere solution of cell-material, but it contains only certain organic substances in solution. The cells, therefore, not only attract materials from out of the cytotblastema, but they must have the faculty of producing chemical changes in its constituent particles. Besides which, all the parts of the cell itself may be chemically altered during the process of its vegetation. The unknown cause of all these phenomena, which we comprise under the term metabolic phenomena of the cells, we will denominate the metabolic power.

The next point which can be proved is, that this power is an attribute of the cells themselves, and that the cytotblastema is passive under

it. We may mention vinous fermentation as an instance of this. A decoction of malt will remain for a long time unchanged; but as soon as some yeast is added to it, which consists partly of entire fungi and partly of a number of single cells, the chemical change immediately ensues. Here the decoction of malt is the cytoblastema; the cells clearly exhibit activity, the cytoblastema, in this instance even a boiled fluid, being quite passive during the change. The same occurs when any simple cells, as the spores of the lower plants, are sown in boiled substances.

In the cells themselves again, it appears to be the solid parts, the cell-membrane and the nucleus, which produce the change. The contents of the cell undergo similar and even more various changes than the external the cytoblastema, and it is at least probable that these changes originate with the solid parts composing the cells, especially the cell-membrane, because the secondary deposits are formed on the inner surface of the cell-membrane, and other precipitates are generally formed in the first instance around the nucleus. It may therefore, on the whole, be said that the solid component particles of the cells possess the power of chemically altering the substances in contact with them.

The substances which result from the transformation of the contents of the cell are different from those which are produced by change in the external cytoblastema. What is the cause of this difference, if the metamorphosing power of the cell-membrane be limited to its immediate neighbourhood merely? Might we not much rather expect that converted substance would be found without distinction on the inner as on the outer surface of the cell-membrane? It might be said that the cell-membrane converts the substance in contact with it without distinction, and that the variety in the products of this conversion depends only upon a difference between the convertible substance contained in the cell and the external cytoblastema. But the question then arises, as to how it happens that the contents of the cell differ from the external cytoblastema. If it be true that the cell-membrane, which at first closely surrounds the nucleus, expands in the course of its growth, so as to leave an interspace between it and the cell, and that the contents of the cell consist of fluid which has entered this space merely by imbibition, they cannot differ essentially from the external cytoblastema. I think therefore that, in order to explain the

distinction between the cell-contents and the external cytotblastema, we must ascribe to the cell-membrane not only the power in general of chemically altering the substances which it is either in contact with, or has imbibed, but also of so separating them that certain substances appear on its inner, and others on its outer surface. The secretion of substances already present in the blood, as, for instance, of urea, by the cells with which the urinary tubes are lined, cannot be explained without such a faculty of the cells. There is, however, nothing so very hazardous in it, since it is a fact that different substances are separated in the decompositions produced by the galvanic pile. It might perhaps be conjectured from this peculiarity of the metabolic phenomena in the cells, that a particular position of the axes of the atoms composing the cell-membrane is essential for the production of these appearances.

Chemical changes occur, however, not only in the cytotblastema and the cell-contents, but also in the solid parts of which the cells are composed, particularly the cell-membrane. Without wishing to assert that there is any intimate connexion between the metabolic power of the cells and galvanism, I may yet, for the sake of making the representation of the process more clear, remark that the chemical changes produced by a galvanic pile are accompanied by corresponding changes in the pile itself.

The more obscure the cause of the metabolic phenomena in the cells is, the more accurately we must mark the circumstances and phenomena under which they occur. One condition to them is a certain temperature, which has a maximum and a minimum. The phenomena are not produced in a temperature below 0° or above 80° R.; boiling heat destroys this faculty of the cells permanently; but the most favorable temperature is one between 10° and 32° R. Heat is evolved by the process itself.

Oxygen, or carbonic acid, in a gaseous form or lightly confined, is essentially necessary to the metabolic phenomena of the cells. The oxygen disappears and carbonic acid is formed, or *vice versa*, carbonic acid disappears, and oxygen is formed. The universality of respiration is based entirely upon this fundamental condition to the metabolic phenomena of the cells. It is so important that, as we shall see further on, even the principal varieties of form in organized bodies are occasioned by this peculiarity of the metabolic process in the cells.

Each cell is not capable of producing chemical changes in every organic substance contained in solution, but only in particular ones. The fungi of fermentation, for instance, effect no changes in any other solutions than sugar; and the spores of certain plants do not become developed in all substances. In the same manner it is probable that each cell in the animal body converts only particular constituents of the blood.

The metabolic power of the cells is arrested not only by powerful chemical actions, such as destroy organic substances in general, but also by matters which chemically are less uncongenial; for instance, concentrated solutions of neutral salts. Other substances, as arsenic, do so in less quantity. The metabolic phenomena may be altered in quality by other substances, both organic and inorganic, and a change of this kind may result even from mechanical impressions on the cells.

Such are the most essential characteristics of the fundamental powers of the cell, so far as they can as yet be deduced from the phenomena. And now, in order to comprehend distinctly in what the peculiarity of the formative process of a cell, and therefore in what the peculiarity of the essential phenomenon in the formation of organized bodies consist, we will compare this process with a phenomenon of inorganic nature as nearly as possible similar to it. Disregarding all that is specially peculiar to the formation of cells, in order to find a more general definition in which it may be included with a process occurring in inorganic nature, we may view it as a process in which a solid body of definite and regular shape is formed in a fluid at the expense of a substance held in solution by that fluid. The process of crystallization in inorganic nature comes also within this definition, and is, therefore, the nearest analogue to the formation of cells.

Let us now compare the two processes, that the difference of the organic process may be clearly manifest. First, with reference to the plastic phenomena, the forms of cells and crystals are very different. The primary forms of crystals are simple, always angular, and bounded by plane surfaces; they are regular, or at least symmetrical, and even the very varied secondary forms of crystals are almost, without exception, bounded by plane surfaces. But manifold as is the form of cells, they have very little resemblance to crystals; round surfaces predominate, and where angles occur, they are never quite sharp, and the polyhedral crystal-like form of many cells results only from

mechanical causes. The structure too of cells and of crystals is different. Crystals are solid bodies, composed merely of layers placed one upon another; cells are hollow vesicles, either single, or several inclosed one within another. And if we regard the membranes of these vesicles as layers, there will still remain marks of difference between them and crystals; these layers are not in contact, but contain fluid between them, which is not the case with crystals; the layers in the cells are few, from one to three only; and they differ from each other in chemical properties, while those of crystals consist of the same chemical substance. Lastly, there is also a great difference between crystals and cells in their mode of growth. Crystals grow by apposition, the new molecules are set only upon the surface of those already deposited, but cells increase also by intussusception, that is to say, the new molecules are deposited also between those already present.

But greatly as these plastic phenomena differ in cells and in crystals, the metabolic are yet more different, or rather they are quite peculiar to cells. For a crystal to grow, it must be already present as such in the solution, and some extraneous cause must interpose to diminish its solubility. Cells, on the contrary, are capable of producing a chemical change in the surrounding fluid, of generating matters which had not previously existed in it as such, but of which only the elements were present in another combination. They therefore require no extraneous influence to effect a change of solubility; for if they can produce chemical changes in the surrounding fluid, they may also produce such substances as could not be held in solution under the existing circumstances, and therefore need no external cause of growth. If a crystal be laid in a pretty strong solution, of a substance similar even to itself, nothing ensues without our interference, or the crystal dissolves completely: the fluid must be evaporated for the crystal to increase. If a cell be laid in a solution of a substance, even different from itself, it grows and converts this substance without our aid. And this it is from which the process going on in the cells (so long as we do not separate it into its several acts) obtains that magical character, to which attaches the idea of Life.

From this we perceive how very different are the phenomena in the formation of cells and of crystals. Meanwhile, however, the points of resemblance between them should not be overlooked. They agree in

this important point, that solid bodies of a certain regular shape are formed in obedience to definite laws at the expense of a substance contained in solution in a fluid; and the crystal, like the cell, is so far an active and positive agent as to cause the substances which are precipitated to be deposited on itself, and nowhere else. We must, therefore, attribute to it as well as to the cell a power to attract the substance held in solution in the surrounding fluid. It does not indeed follow that these two attractive powers, the power of crystallization—to give it a brief title—and the plastic power of the cells, are essentially the same. This could only be admitted, if it were proved that both powers acted according to the same laws. But this is seen at the first glance to be by no means the case: the phenomena in the formation of cells and crystals, are, as we have observed, very different, even if we regard merely the plastic phenomena of the cells, and leave their metabolic power (which may possibly arise from some other peculiarity of organic substance) for a time entirely out of the question.

Is it, however, possible that these distinctions are only secondary, that the power of crystallization and the plastic power of the cells are identical, and that an original difference can be demonstrated between the substance of cells and that of crystals, by which we may perceive that the substance of cells must crystallize as cells according to the laws by which crystals are formed, rather than in the shape of the ordinary crystals? It may be worth while to institute such an inquiry.

In seeking such a distinction between the substance of cells and that of crystals, we may say at once that it cannot consist in anything which the substance of cells has in common with those organic substances which crystallize in the ordinary form. Accordingly, the more complicated arrangement of the atoms of the second order in organic bodies cannot give rise to this difference; for we see in sugar, for instance, that the mode of crystallization is not altered by this chemical composition.

Another point of difference by which inorganic bodies are distinguished from at least some of the organic bodies, is the faculty of imbibition. Most organic bodies are capable of being infiltrated by water, and in such a manner that it penetrates not so much into the interspaces between the elementary tissues of the body, as into the simple structureless tissues, such as areolar tissue, &c.; so that they form an homogeneous mixture, and we can neither distinguish par-

ticles of organic matter, nor interspaces filled with water. The water occupies the infiltrated organic substances, just as it is present in a solution, and there is as much difference between the capacity for imbibition and capillary permeation, as there is between a solution and the phenomena of capillary permeation. When water soaks through a layer of glue, we do not imagine it to pass through pores, in the common sense of the term; and this is just the condition of all substances capable of imbibition. They possess, therefore, a double nature, they have a definite form like solid bodies; but like fluids, on the other hand, they are also permeable by anything held in solution. As a specifically lighter fluid poured on one specifically heavier so carefully as not to mix with it, yet gradually penetrates it, so also, every solution, when brought into contact with a membrane already infiltrated with water, bears the same relations to the membrane, as though it were a solution. And crystallization being the transition from the fluid to the solid state, we may conceive it possible, or even probable, that if bodies, capable of existing in an intermediate state between solid and fluid could be made to crystallize, a considerable difference would be exhibited from the ordinary mode of crystallization. In fact, there is nothing, which we call a crystal, composed of substance capable of imbibition; and even among organized substances, crystallization takes place only in those which are capable of imbibition, as fat, sugar, tartaric acid, &c. The bodies capable of imbibition, therefore, either do not crystallize at all, or they do so under a form so different from the crystal that they are not recognized as such.

Let us inquire what would most probably ensue if material capable of imbibition crystallized according to the ordinary laws, what varieties from the common crystals would be most likely to show themselves, assuming only that the solution has permeated through the parts of the crystal already formed, and that new molecules can therefore be deposited between them. The ordinary crystals increase only by apposition; but there may be an important difference in the mode of this apposition. If the molecules were all deposited symmetrically one upon another, we might indeed have a body of a certain external form like a crystal; but it would not have the structure of one, it would not consist of layers. The existence of this laminated structure in crystals presupposes a double kind of apposition of their molecules; for in each layer the newly-deposited molecules coalesce, and become

continuous with those of the same layer already present; but those molecules which form the adjacent surfaces of two layers do not coalesce. This is a remarkable peculiarity in the formation of crystals, and we are quite ignorant of its cause. We cannot yet perceive why the new molecules, which are being deposited on the surface of a crystal (already formed up to a certain point), do not coalesce and become continuous with those already deposited, like the molecules in each separate layer, instead of forming, as they do, a new layer; and why this new layer does not constantly increase in thickness, instead of producing a second layer around the crystal, and so on. In the meantime we can do no more than express the fact in the form of a law, that the coalescing molecules are deposited rather along the surface beside each other, than in the thickness upon one another, and thus, as the breadth of the layer depends upon the size of the crystal, so also the layer can attain only a certain thickness, and beyond this, the molecules which are being deposited cannot coalesce with it, but must form a new layer.

If we now assume that bodies capable of imbibition could also crystallize, the two modes of junction of the molecules should be shown also by them. Their structure should also be laminated, at least there is no perceptible reason for a difference in this particular, as the very fact of layers being formed in common crystals shows that the molecules need not be all joined together in the most exact manner possible. The closest possible conjunction of the molecules takes place only in the separate layers. In the common crystals this occurs by apposition of the new molecules on the surface of those present and coalescence with them. In bodies capable of imbibition, a much closer union is possible, because in them the new molecules may be deposited by intussusception between those already present. It is scarcely, therefore, too bold an hypothesis to assume, that when bodies capable of imbibition crystallize, their separate layers would increase by intussusception; and that this does not happen in ordinary crystals, simply because it is impossible.

Let us then imagine a portion of the crystal to be formed: new molecules continue to be deposited, but do not coalesce with the portion of the crystal already formed; they unite with one another only, and form a new layer, which, according to analogy with the common crystals, may invest either the whole or a part of the crystal. We will

assume that it invests the entire crystal. Now, although this layer be formed by the deposition of new molecules between those already present instead of by apposition, yet this does not involve any change in the law, in obedience to which the deposition of the coalescing molecules goes on more vigorously in two directions, that is, along the surface, than it does in the third direction corresponding to the thickness of the layer; that is to say, the molecules which are deposited by intussusception between those already present, must be deposited much more vigorously between those lying together along the surface of the layer than between those which lie over one another in its thickness. This deposition of molecules side by side is limited in common crystals by the size of the crystal, or by that of the surface on which the layer is formed; the coalescence of molecules therefore ceases as regards that layer, and a new one begins. But if the layers grow by intussusception in crystals capable of imbibition, there is nothing to prevent the deposition of more molecules between those which lie side by side upon the surface, even after the lamina has invested the whole crystal; it may continue to grow without the law by which the new molecules coalesce requiring to be altered. But the consequence is, that the layer becomes, in the first instance more condensed, that is, more solid substance is taken into the same space; and afterwards it will expand and separate from the completed part of the crystal so as to leave a hollow space between itself and the crystal; this space fills with fluid by imbibition, and the first-formed portion of the crystal adheres to a spot on its inner surface. Thus, in bodies capable of imbibition, instead of a new layer attached to the part of the crystal already formed, we obtain a hollow vesicle. At first this must have the shape of the body of the crystal around which it is formed, and must, therefore, be angular, if the crystal is angular. If, however, we imagine this layer to be composed of soft substance capable of imbibition, we may readily comprehend how such a vesicle must very soon become round or oval. But the first-formed part of the crystal also consists of substance capable of imbibition, so that it is very doubtful whether it must have an angular form at all. In common crystals atoms of some one particular substance are deposited together, and we can understand how a certain angular form of the crystal may result if these atoms have a certain form, or if in certain axes they attract each other differently. But in bodies capable of imbibition, an atom of one sub-

stance is not set upon another atom of the same substance, but atoms of water come between; atoms of water, which are not united with an atom of solid substance, so as to form a compound atom, as in the water of crystallization, but which exist in some other unknown manner between the atoms of solid substance. It is not possible, therefore, to determine whether that part of the crystal which is first formed must have an angular figure or not.

An ordinary crystal consists of a number of laminae; when so small as to be but just discernible, it has the form which the whole crystal afterwards exhibits, at least as far as regards the angles; we must therefore suppose that the first layer is formed around a very small corpuscle, which is of the same shape as the subsequent crystal. We will call this the primitive corpuscle. It is doubtful what may be the shape of this corpuscle in the crystals which are capable of imbibition. The first layer, then, is formed around the corpuscle in the way mentioned; it grows by intussusception, and thus forms a hollow, round or oval vesicle, to the inner surface of which the primitive corpuscle adheres. As all the new molecules that are being deposited may be placed in this layer without any alteration being required in the law which regulates the coalescence of the molecules during crystallization, we must conclude that it remains the only layer, and becomes greatly expanded, so as to represent all the layers of an ordinary crystal. It is, however, a question whether there may not exist some reasons why several layers can be formed. We can certainly conceive such to be the case. The quantity of the solid substance that must crystallize in a given time, depends upon the concentration of the fluid; the number of molecules that may, in accordance with the law already mentioned, be deposited in the layer in a given time depends upon the quantity of the solution which can penetrate the membrane by imbibition during that time. If in consequence of the concentration of the fluid there must be more precipitated in the time than can penetrate the membrane, it can only be deposited as a new layer on the outer surface of the vesicle. When this second layer is formed, the new molecules are deposited in it, and it rapidly becomes expanded into a vesicle, on the inner surface of which the first vesicle lies with its primitive corpuscle. The first vesicle now either does not grow at all, or at any rate much more slowly, and then only when the endosmosis into the cavity of the second vesicle proceeds so rapidly that all

that might be precipitated while passing through it, is not deposited. The second vesicle, when it is developed at all, must needs be developed relatively with more rapidity than the first; for as the solution is in the most concentrated state at the beginning, the necessity for the formation of a second layer then occurs sooner; but when it is formed, the concentration of the fluid is diminished, and this necessity occurs either later or not at all. It is possible, however, that even a third, or fourth, and more, may be formed; but the outermost layer must always be relatively the most vigorously developed; for when the concentration of the solution is only so strong, that all that must be deposited in a certain time, can be deposited in the outermost layer, it is all applied to the increase of this layer.

Such, then, would be the phenomena under which substances capable of imbibition would probably crystallize, if they did so at all. I say probably, for our incomplete knowledge of crystallization and the faculty of imbibition, does not as yet admit of our saying anything positively *a priori*. It is, however, obvious that these are the principal phenomena attending the formation of cells. They consist always of substance capable of imbibition; the first part formed is a small corpuscle, not angular (nucleolus), around this a lamina is deposited (nucleus), which advances rapidly in its growth, until a second lamina (cell) is formed around it. This second now grows more quickly and expands into a vesicle, as indeed often happens with the first layer. In some rarer instances only one layer is formed; in others, again, there are three. The only other difference in the formation of cells is, that the separate layers do not consist of the same chemical substance, while a common crystal is always composed of one material. In instituting a comparison, therefore, between the formation of cells and crystallization, the above-mentioned differences in form, structure, and mode of growth fall altogether to the ground. If crystals were formed from the same substance as cells, they would probably, in these respects, be subject to the same conditions as the cells. Meanwhile the metabolic phenomena, which are entirely absent in crystals, still indicate essential distinctions.

Should this important difference between the mode of formation of cells and crystals lead us to deny all intimate connexion of the two processes, the comparison of the two may serve at least to give a clear representation of the cell-life. The following may be conceived to be

the state of the matter : the material of which the cells are composed is capable of producing chemical changes in the substance with which it is in contact, just as the well-known preparation of platinum converts alcohol into acetic acid. This power is possessed by every part of the cell. Now, if the cytoblastema be so changed by a cell already formed, that a substance is produced which cannot become attached to that cell, it immediately crystallizes as the central nucleus of a new cell. And then this converts the cytoblastema in the same manner. A portion of that which is converted may remain in the cytoblastema in solution, or may crystallize as the commencement of new cells ; another portion, the cell-substance, crystallizes around the central corpuscle. The cell-substance is either soluble in the cytoblastema, and crystallizes from it, so soon as the latter becomes saturated with it ; or else it is insoluble, and crystallizes at the time of its formation, according to the laws of crystallization of bodies capable of imbibition mentioned above, forming in this manner one or more layers around the central corpuscle, and so on. If we conceive the above to represent the mode of formation of cells, we regard the plastic power of the cells as identical with the power by which crystals grow. According to the foregoing description of the crystallization of bodies capable of imbibition, the most important plastic phenomena of the cells are certainly satisfactorily explained. But let us see if this comparison agrees with all the characteristics of the plastic power of the cells.

The attractive power of the cells does not always operate symmetrically ; the deposition of new molecules may be more vigorous in particular spots, and thus produce a change in the form of the cell. This is quite analogous to what happens in crystals ; for although in them an angle is never altered, there may be much more material deposited on some surfaces than on others ; and thus, for instance, a quadrilateral prism may be formed out of a cube. In this case new layers are deposited on one, or on two opposite sides of a cube. Now, if one layer in cells represent a number of layers in a common crystal, it may be easily perceived that instead of several new layers being formed on two opposite surfaces of a cell, the one layer would grow more at those spots, and thus a round cell would be elongated into a fibre ; and so with the other changes of form. Division of the cells can have no analogue in common crystals, because that which is once deposited is incapable of any further change. But this phenomenon may be

made to accord with the representation of crystals capable of imbibition . . . And if we ascribe to a layer of a crystal capable of imbibition the power of producing chemical changes in organic substances, we can very well understand also the origin of secondary deposits on its inner surface as they occur in cells. For if, in accordance with the laws of crystallization, the lamina has become expanded into a vesicle, and its cavity has become filled by imbibition with a solution of organic substance, there may be materials formed by means of the converting influence of the lamina, which cannot any longer be held in solution. These may, then, either crystallize within the vesicle, as new crystals capable of imbibition under the form of cells; or if they are allied to the substance of the vesicle, they may so crystallize as to form part of the system of the vesicle itself: the latter may occur in two ways, the new matters may be applied to the increase of the vesicle, or they may form new layers on its inner surface from the same cause which led to the first formation of the vesicle itself as a layer. In the cells of plants these secondary deposits have a spiral arrangement. This is a very important fact, though the laws of crystallization do not seem to account for the absolute necessity of it. If, however, it could be mathematically proved from the laws of the crystallization of inorganic bodies, that under the altered circumstances in which bodies capable of imbibition are placed, these deposits must be arranged in spiral forms, it might be asserted without hesitation that the plastic power of cells and the fundamental powers of crystals are identical.

We come now, however, to some peculiarities in the plastic power of cells, to which we might, at first sight, scarcely expect to find anything analogous in crystals. The attractive power of the cells manifests a certain degree of election in its operation; it does not attract every substance present in the cytoblastema, but only particular ones; and here a muscle-cell, there a fat-cell, is generated from the same fluid, the blood. Yet crystals afford us an example of a precisely similar phenomenon, and one which has already been frequently adduced as analogous to assimilation. If a crystal of nitre be placed in a solution of nitre and sulphate of soda, only the nitre crystallizes; when a crystal of sulphate of soda is put in, only the sulphate of soda crystallizes. Here, therefore, there occurs just the same selection of the substance to be attracted.



We observed another law attending the development of the plastic phenomena in the cells, viz. that a more concentrated solution is requisite for the first formation of a cell than for its growth when already formed, a law upon which the difference between organized and unorganized tissues is based. In ordinary crystallization the solution must be more than saturated for the process to begin. But when it is over, there remains a mother lye, according to Thénard, which is no longer saturated at the same temperature. This phenomenon accords precisely with the cells; it shows that a more concentrated solution is requisite for the commencement of crystallization than for the increase of a crystal already formed. The fact has indeed been disputed by Thomson; but if, in the undisputed experiment quoted above, the crystal of sulphate of soda attracts the dissolved sulphate of soda rather than the dissolved nitre, and *vice versa*, the crystal of nitre attracts the dissolved nitre more than the dissolved sulphate of soda, it follows that a crystal does attract a salt held in solution, because the experiment proves that there are degrees of this attraction. But if there be such an attraction exerted by a crystal, then the introduction of a crystal into a solution of a salt, affords an efficient cause for the deposition of this salt, which does not exist when no crystal is introduced. The solution must therefore be more concentrated in the latter case than in the former, though the difference be so slight as not to be demonstrable by experiment. It would not, however, be superfluous to repeat the experiments. In the instance of crystals capable of imbibition, this difference may be considerably augmented, since the attraction of molecules may increase perhaps considerably by the penetrating of the solution between those already deposited.

We see then how all the plastic phenomena in the cells may be compared with phenomena which, in accordance with the ordinary laws of crystallization, would probably appear if bodies capable of imbibition could be brought to crystallize. So long as the object of such a comparison were merely to render the representation of the process by which cells are formed more clear, there could not be much urged against it; it involves nothing hypothetical, since it contains no explanation; no assertion is made that the fundamental power of the cells really has something in common with the power by which crystals are formed. We have, indeed, compared the growth of organisms with crystallization, in so far as in both cases solid substances are deposited

from a fluid, but we have not therefore asserted the identity of the fundamental powers. So far we have not advanced beyond the data, beyond a certain simple mode of representing the facts.

The question is, however, whether the exact accordance of the phenomena would not authorize us to go further. If the formation and growth of the elementary particles of organisms have nothing more in common with crystallization than merely the deposition of solid substances from out of a fluid, there is certainly no reason for assuming any more intimate connexion of the two processes. But we have seen, first, that the laws which regulate the deposition of the molecules forming the elementary particles of organisms are the same for all elementary parts; that there is a common principle in the development of all elementary parts, namely, that of the formation of cells; it was then shown that the power which induced the attachment of the new molecules did not reside in the entire organism, but in the separated elementary particles (this we called the plastic power of the cells); lastly, it was shown that the laws, according to which the new molecules combine to form cells, are (so far as our incomplete knowledge of the laws of crystallization admits of our anticipating their probability) the same as those by which substances capable of imbibition would crystallize. Now the cells do, in fact, consist only of material capable of imbibition; should we not then be justified in putting forth the proposition, that the formation of the elementary parts of organisms is nothing but a crystallization of substance, capable of imbibition, and the organism nothing but an aggregate of such crystals capable of imbibition?

To advance so important a point as absolutely true, would certainly need the clearest proof; but it cannot be said that even the premises which have been set forth have in all points the requisite force. For too little is still known of the cause of crystallization to predict with safety (as was attempted above) what would follow if a substance capable of imbibition were to crystallize. And if these premises were allowed, there are two other points which must be proved in order to establish the proposition in question: 1. That the metabolic phenomena of the cells, which have not been referred to in the foregoing argument, are as much the necessary consequence of the faculty of imbibition, or of some other peculiarity of the substance of cells, as the plastic phenomena are. 2. That if a number of crystals capable of imbibition

tion are formed, they must combine according to certain laws so as to form a systematic whole, similar to an organism. Both these points must be clearly proved, in order to establish the truth of the foregoing view. But it is otherwise if this view be adduced merely as an hypothesis, which may serve as a guide for new investigations. In such case the inferences are sufficiently probable to justify such an hypothesis, if only the two points just mentioned can be shown to accord with it.

With reference to the first of these points, it would certainly be impossible, in our ignorance as to the cause of chemical phenomena in general, to prove that a crystal capable of imbibition must produce chemical changes in substances surrounding it; but then we could not infer, from the manner in which spongy platinum is formed, that it would act so peculiarly upon oxygen and hydrogen. But in order to render this view tenable as a possible hypothesis, it is only necessary to see that it may be a consequence. It cannot be denied that it may: there are several reasons for it, though they certainly are but weak. For instance, since all cells possess this metabolic power, it is more likely to depend on a certain position of the molecules, which in all probability is essentially the same in all cells, than on the chemical combination of the molecules, which is very different in different cells. The presence, too, of different substances on the inner and outer surface of the cell-membrane in some measure implies that a certain direction of the axes of the atoms may be essential to the metabolic phenomena of the cells. I think, therefore, that the cause of the metabolic phenomena resides in that definite mode of arrangement of the molecules which occurs in crystals, combined with the capacity which the solution has to penetrate between these regularly deposited molecules (by means of which, presuming the molecules to possess polarity, a sort of galvanic pile will be formed), and that the same phenomena would be observed in an ordinary crystal, if it could be rendered capable of imbibition. And then perhaps the differences of quality in the metabolic phenomena depend upon their chemical composition.

In order to render tenable the hypothesis contained in the second point, it is merely necessary to show that crystals capable of imbibition can unite with one another according to certain laws. If at their first formation all crystals were isolated, if they held no relation whatever

to each other, the view would leave entirely unexplained how the elementary parts of organisms, that is, the crystals in question, become united to form a whole. It is therefore necessary to show that crystals do unite with each other according to certain laws, in order to perceive, at least, the possibility of their uniting also to form an organism, without the need of any further combining power. But there are many crystals in which a union of this kind, according to certain laws, is indisputable; indeed they often form a whole, so like an organism in its entire form, that groups of crystals are known in common life by the names of flowers, trees, etc. I need only refer to the ice-flowers on the windows, or to the lead-tree, etc. In such instances a number of crystals arrange themselves in groups around others, which form an axis. If we consider the contact of each crystal with the surrounding fluid to be an indispensable condition to the growth of crystals which are not capable of imbibition, but that those which are capable of imbibition, in which the solution can penetrate whole layers of crystals, do not require this condition, we perceive that the similarity between organisms and these aggregations of crystals is as great as could be expected with such difference of substance. As most cells require for the production of their metabolic phenomena, not only their peculiar nutrient fluid, but also the access of oxygen and the power of exhaling carbonic acid, or *vice versa*; so, on the other hand, organisms in which there is no circulation of respiratory fluid, or in which at least it is not sufficient, must be developed in such a way as to present as extensive a surface as possible to the atmospheric air. This is the condition of plants, which require for their growth that the individual cells should come into contact with the surrounding medium in a similar manner, if not in the same degree as occurs in a crystal tree, and in them indeed the cells unite into a whole organism in a form much resembling a crystal tree. But in animals the circulation renders the contact of the individual cells with the surrounding medium superfluous, and they may have more compact forms, even though the laws by which the cells arrange themselves are essentially the same.

The view then that organisms are nothing but the form under which substances capable of imbibition crystallize, appears to be compatible with the most important phenomena of organic life, and may be so far admitted, that it is a possible hypothesis; or attempt towards an ex-

planation of these phenomena. It involves very much that is uncertain and paradoxical, but I have developed it in detail, because it may serve as a guide for new investigations. For even if no relation between crystallization and the growth of organisms be admitted in principle, this view has the advantage of affording a distinct representation of the organic processes; an indispensable requisite for the institution of new inquiries in a systematic manner, or for testing by the discovery of new facts a mode of explanation which harmonizes with phenomena already known.

HERMANN VON HELMHOLTZ

1821-1894

Hermann von Helmholtz, born at Potsdam, Prussia, August 31, 1821, studied medicine at the University of Berlin, from which he received his degree in 1842. He then entered the German Army as surgeon and in 1847 published his paper on "The Conservation of Energy," which summarized historically the development of the idea. In 1849 he was appointed professor of physiology and general pathology at Königsberg. In 1855 he was called to Bonn, and in 1858 was elected to the chair of physiology at Heidelberg.

In 1851 he invented the ophthalmoscope and later at Heidelberg he continued his researches in the subject of sight, and also cleared up the problem of the mechanical causes of sound. In 1871 he was appointed professor of physics at the University of Berlin, where he remained until his death, September 8, 1894.

THE CONSERVATION OF ENERGY *

A new conquest of very general interest has been recently made by natural philosophy. In the following pages I will endeavour to give a notion of the nature of this conquest. It has reference to a new and universal natural law, which rules the action of natural forces in their mutual relations towards each other, and is as influential on our theoretic views of natural processes as it is important in their technical applications.

Among the practical arts which owe their progress to the development of the natural sciences, from the conclusion of the middle ages downwards, practical mechanics, aided by the mathematical science which bears the same name, was one of the most prominent. The

* Translated from *Über die Erhaltung der Kraft* (Berlin, 1847).

character of the art was, at the time referred to, naturally very different from its present one. Surprised and stimulated by its own success, it thought no problem beyond its power, and immediately attacked some of the most difficult and complicated. Thus it was attempted to build automaton figures which should perform the functions of men and animals. The wonder of the last century was Vaucanson's duck, which fed and digested its food; the flute player of the same artist, which moved all its fingers correctly; the writing boy of the older, and the pianoforte player of the younger Droz: which latter, when performing, followed its hands with its eyes, and at the conclusion of the piece bowed courteously to the audience. That men like those mentioned, whose talent might bear comparison with the most inventive heads of the present age, should spend so much time in the construction of these figures, which we at present regard as the merest trifles, would be incomprehensible, if they had not hoped in solemn earnest to solve a great problem. The writing boy of the elder Droz was publicly exhibited in Germany some years ago. Its wheel-work is so complicated, that no ordinary head would be sufficient to decipher its manner of action. When, however, we are informed that this boy and its constructor, being suspected of the black art, lay for a time in the Spanish Inquisition, and with difficulty obtained their freedom, we may infer that in those days even such a toy appeared great enough to excite doubts as to its natural origin. And though these artists may not have hoped to breathe into the creature of their ingenuity a soul gifted with moral completeness, still there were many who would be willing to dispense with the moral qualities of their servants if, at the same time, their immoral qualities could also be got rid of; and accept, instead of the mutability of flesh and bones, services which should combine the regularity of a machine with the durability of brass and steel. The object, therefore, which the inventive genius of the past century placed before it with the fullest earnestness, and not as a piece of amusement merely, was boldly chosen, and was followed up with an expenditure of sagacity which has contributed not a little to enrich the mechanical experience which a later time knew how to take advantage of. We no longer seek to build machines which shall fulfil the thousand services required of one man, but desire, on the contrary, that a machine shall perform one service, but shall occupy in doing it the place of a thousand men.

From these efforts to imitate living creatures, another idea, also by a misunderstanding, seems to have developed itself, which, as it were, formed the new philosopher's stone of the seventeenth and eighteenth centuries. It was now the endeavour to construct a perpetual motion machine. Under this term was understood a machine which, without being wound up, without consuming in the working of it, falling water, wind or any other natural force, should still continue in motion, the motive power being perpetually supplied by the machine itself. Beasts and human beings seemed to correspond to the idea of such an apparatus, for they moved themselves energetically and incessantly as long as they lived, were never wound up, and nobody set them in motion. A connection between the taking in of nourishment and the development of force did not make itself apparent. The nourishment seemed only necessary to grease, as it were, the wheel-work of the animal machine, to replace what was used up, and to renew the old. The development of force out of itself seemed to be the essential peculiarity, the real quintessence of organic life. If, therefore, men were to be constructed, a perpetual motion must first be found.

Another hope also seemed to take up incidentally the second place, which, in our wiser age, would certainly have claimed the first rank in the thoughts of men. The perpetual motion was to produce work inexhaustibly without corresponding consumption, that is to say, out of nothing. Work, however, is money. Here, therefore, the practical problem which the cunning heads of all centuries have followed in the most diverse ways, namely, to fabricate money out of nothing, invited solution. The similarity with the philosopher's stone sought by the ancient chemists was complete. That also was thought to contain the quintessence of organic life, and to be capable of producing gold.

The spur which drove men to inquiry was sharp, and the talent of some of the seekers must not be estimated as small. The nature of the problem was quite calculated to entice poring brains, to lead them round a circle for years, deceiving ever with new expectations, which vanished upon nearer approach, and finally reducing these dupes of hope to open insanity. The phantom could not be grasped. It would be impossible to give a history of these efforts, as the clearer heads, among whom the elder Droz must be ranked, convinced themselves of the futility of their experiments, and were naturally not inclined to speak much about them. Bewildered intellects, however, proclaimed

often enough that they had discovered the grand secret; and as the incorrectness of their proceedings was always speedily manifest, the matter fell into bad repute, and the opinion strengthened itself more and more that the problem was not capable of solution; one difficulty after another was brought under the dominion of mathematical mechanics, and finally a point was reached where it could be proved that, at least by the use of pure mechanical forces, no perpetual motion could be generated.

We have here arrived at the idea of the driving force or power of a machine, and shall have much to do with it in future. I must, therefore, give an explanation of it. The idea of work is evidently transferred to machines by comparing their arrangements with those of men and animals to replace which they were applied. We still reckon the work of steam engines according to horse-power. The value of manual labor is determined partly by the force which is expended in it (a strong laborer is valued more highly than a weak one), partly, however, by the skill which is brought into action. A machine, on the contrary, which executes work skilfully, can always be multiplied to any extent; hence its skill has not the high value of human skill in domains where the latter cannot be supplied by machines. Thus the idea of the quantity of work in the case of machines has been limited to the consideration of the expenditure of force; this was the more important, as indeed most machines are constructed for the express purpose of exceeding, by the magnitude of their effects, the powers of men and animals. Hence, in a mechanical sense, the idea of work is become identical with that of the expenditure of force, and in this way I will apply it.

How, then, can we measure this expenditure, and compare it in the case of different machines?

I must here conduct you a portion of the way—as short a portion as possible—over the uninviting field of mathematico-mechanical ideas, in order to bring you to a point of view from which a more rewarding prospect will open. And though the example which I shall here choose, namely, that of a water-mill with iron hammer, appears to be tolerably romantic, still, alas, I must leave the dark forest valley, the spark-emitting anvil, and the black Cyclops wholly out of sight, and beg a moment's attention to the less poetic side of the question, namely, the machinery. This is driven by a water-wheel, which in its turn is

set in motion by the falling water. The axle of the water-wheel has at certain places small projections, thumbs, which, during the rotation, lift the heavy hammer and permit it to fall again. The falling hammer belabors the mass of metal, which is introduced beneath it. The work therefore done by the machine consists, in this case, in the lifting of the hammer, to do which the gravity of the latter must be overcome. The expenditure of force will, in the first place, other circumstances being equal, be proportioned to the weight of the hammer; it will, for example, be double when the weight of the hammer is doubled. But the action of the hammer depends not upon its weight alone, but also upon the height from which it falls. If it falls through two feet, it will produce a greater effect than if it falls through only one foot. It is, however, clear that if the machine, with a certain expenditure of force, lifts the hammer a foot in height, the same amount of force must be expended to raise it a second foot in height. The work is therefore not only doubled when the weight of the hammer is increased twofold, but also when the space through which it falls is doubled. From this it is easy to see that the work must be measured by the product of the weight into the space through which it ascends. And in this way, indeed, do we measure in mechanics.

The unit of work is a foot-pound, that is, a pound weight, raised to the height of one foot.

While the work in this case consists in the raising of the heavy hammer-head, the driving force which sets the latter in motion is generated by falling water. It is not necessary that the water should fall vertically, it can also flow in a moderately inclined bed; but it must always, where it has water-mills to set in motion, move from a higher to a lower position. Experiment and theory coincided in teaching, that when a hammer of a hundred weight is to be raised one foot, to accomplish this at least a hundred weight of water must fall through the space of one foot; or what is equivalent to this, two hundred weight must fall full half a foot, or four hundred weight a quarter of a foot, etc. In short, if we multiply the weight of the falling water by the height through which it falls, and regard, as before, the product as the measure of the work, then the work performed by the machine in raising the hammer can, in the most favourable case, be only equal to the number of foot-pounds of water which have fallen in the same

time. In practice, indeed, this ratio is by no means attained; a great portion of the work of the falling water escapes unused, inasmuch as part of the force is unwillingly sacrificed for the sake of obtaining greater speed.

I will further remark, that this relation remains unchanged whether the hammer is driven immediately by the axle of the wheel, or whether—by the intervention of wheel-work, endless screws, pulleys, ropes—the motion is transferred to the hammer. We may, indeed, by such arrangements, succeed in raising a hammer of ten hundred weight, when by the first simple arrangement, the elevation of a hammer of one hundred weight might alone be possible; but either this heavier hammer is raised to only one-tenth of the height, or tenfold the time is required to raise it to the same height; so that, however we may alter, by the interposition of machinery, the intensity of the acting force, still in a certain time, during which the mill-stream furnishes us with a definite quantity of water, a certain definite quantity of work, and no more, can be performed.

Our machinery, therefore, has, in the first place, done nothing more than make use of the gravity of the falling water in order to overpower the gravity of the hammer, and to raise the latter. When it has lifted the hammer to the necessary height, it again liberates it, and the hammer falls upon the metal mass which is pushed beneath it. But why does the falling hammer here exercise a greater force than when it is permitted simply to press with its own weight on the mass of metal? Why is its power greater as the height from which it falls is increased? We find, in fact, that the work performed by the hammer is determined by its velocity. In other cases, also, the velocity of moving masses is a means of producing great effects. I only remind you of the destructive effects of musket-bullets, which, in a state of rest, are the most harmless things in the world. I remind you of the wind-mill, which derives its force from the moving air. It may appear surprising that motion, which we are accustomed to regard as a non-essential and transitory endowment of bodies, can produce such great effects. But the fact is, that motion appears to us, under ordinary circumstances, transitory, because the movement of all terrestrial bodies is resisted perpetually by other forces, friction, resistance of the air, etc., so that motion is incessantly weakened and finally neutralized. A body, however, which is opposed by no resisting force,

when once set in motion, moves onward eternally with undiminished velocity. Thus we know that the planetary bodies have moved without change, through space, for thousands of years. Only by resisting forces can motion be diminished or destroyed. A moving body, such as the hammer or the musket-ball, when it strikes against another, presses the latter together, or penetrates it, until the sum of the resisting forces which the body struck presents to its pressure, or to the separation of its particles, is sufficiently great to destroy the motion of the hammer or of the bullet. The motion of a mass regarded as taking the place of working force is called the living force (*vis viva*) of the mass. The word "living" has of course here no reference whatever to living beings, but is intended to represent solely the force of the motion as distinguished from the state of unchanged rest—from the gravity of a motionless body, for example, which produces an incessant pressure against the surface which supports it, but does not produce any motion.

In the case before us, therefore, we had first power in the form of a falling mass of water, then in the form of a lifted hammer, and, thirdly, in the form of the living force of the fallen hammer. We should transform the third form into the second, if we, for example, permitted the hammer to fall upon a highly elastic steel beam strong enough to resist the shock. The hammer would rebound, and in the most favourable case would reach a height equal to that from which it fell, but would never rise higher. In this way its mass would ascend: and at the moment when its highest point has been attained, it would represent the same number of raised foot-pounds as before it fell, never a greater number; that is to say, living force can generate the same amount of work as that expended in its production. It is therefore equivalent to this quantity of work.

Our clocks are driven by means of sinking weights, and our watches by means of the tension of springs. A weight which lies on the ground, an elastic spring which is without tension, can produce no effects; to obtain such we must first raise the weight or impart tension to the spring, which is accomplished when we wind up our clocks and watches. The man who winds the clock or watch communicates to the weight or to the spring a certain amount of power, and exactly so much as is thus communicated is gradually given out again during the following twenty-four hours, the original force being thus slowly

consumed to overcome the friction of the wheels and the resistance which the pendulum encounters from the air. The wheel-work of the clock therefore exhibits no working force which was not previously communicated to it, but simply distributes the force given to it uniformly over a longer time.

Into the chamber of an air-gun we squeeze, by means of a condensing air-pump, a great quantity of air. When we afterwards open the cock of a gun and admit the compressed air into the barrel, the ball is driven out of the latter with a force similar to that exerted by ignited powder. Now we may determine the work consumed in the pumping-in of the air, and the living force which, upon firing, is communicated to the ball, but we shall never find the latter greater than the former. The compressed air has generated no working force, but simply gives to the bullet that which has been previously communicated to it. And while we have pumped for perhaps a quarter of an hour to charge the gun, the force is expended in a few seconds when the bullet is discharged; but because the action is compressed into so short a time, a much greater velocity is imparted to the ball than would be possible to communicate to it by the unaided effort of the arm in throwing it.

From these examples you observe, and the mathematical theory has corroborated this for all purely mechanical, that is to say, for moving forces, that all our machinery and apparatus generate no force, but simply yield up the power communicated to them by natural forces—falling water, moving wind, or by the muscles of men and animals. After this law had been established by the great mathematicians of the last century, a perpetual motion, which should make only use of pure mechanical forces, such as gravity, elasticity, pressure of liquids and gases, could only be sought after by bewildered and ill-instructed people. But there are still other natural forces which are not reckoned among the purely moving forces—heat, electricity, magnetism, light, chemical forces, all of which nevertheless stand in manifold relation to mechanical processes. There is hardly a natural process to be found which is not accompanied by mechanical actions, or from which mechanical work may not be derived. Here the question of a perpetual motion remained open; the decision of this question marks the progress of modern physics.

In the case of the air-gun, the work to be accomplished in the pro-

pulsion of the ball was given by the arm of the man who pumped in the air. In ordinary firearms, the condensed mass of air which propels the bullet is obtained in a totally different manner, namely, by the combustion of the powder. Gunpowder is transformed by combustion for the most part into gaseous products, which endeavor to occupy a much larger space than that previously taken by the volume of the powder. Thus, you see, that, by the use of gunpowder, the work which the human arm must accomplish in the case of the air-gun is spared.

In the mightiest of our machines, the steam engine, it is a strongly compressed aeriform body, water, vapour, which, by its effort to expand, sets the machine in motion. Here, also, we do not condense the steam by means of an external mechanical force, but by communicating heat to a mass of water in a closed boiler, we change this water into steam, which, in consequence of the limits of the space, is developed under strong pressure. In this case, therefore, it is the heat communicated which generates the mechanical force. The heat thus necessary for the machine we might obtain in many ways; the ordinary method is to procure it from the combustion of coal.

Combustion is a chemical process. A particular constituent of our atmosphere, oxygen, possesses a strong force of attraction, or, as it is named in chemistry, a strong affinity for the constituents of the combustible body, which affinity, however, in most cases, can only exert itself at high temperatures. As soon as a portion of the combustible body, for example, the coal, is sufficiently heated, the carbon unites itself with great violence to the oxygen of the atmosphere and forms a peculiar gas, carbonic acid, the same which we see foaming from beer and champagne. By this combination, light and heat are generated; heat is generally developed by any combination of two bodies of strong affinity for each other; and when the heat is intense enough, light appears. Hence, in the steam engine, it is chemical processes and chemical forces which produce the astonishing work of these machines. In like manner the combustion of gunpowder is a chemical process which, in the barrel of the gun, communicates living force to the bullet.

While now the steam engine develops for us mechanical work out of heat, we can conversely generate heat by mechanical forces. A skilful blacksmith can render an iron wedge red hot by hammer-

ing. The axes of our carriages must be protected, by careful greasing, from ignition through friction. Even lately this property has been applied on a large scale. In some factories, where a surplus of water power is at hand, this surplus is applied to cause a strong iron plate to rotate swiftly upon another, so that they become strongly heated by friction. The heat so obtained warms the room, and thus a stove without fuel is provided. Now, could not the heat generated by the plates be applied to a small steam engine, which in its turn should be able to keep the rubbing plates in motion? The perpetual motion would thus be at length found. This question might be asked, and could not be decided by the older mathematico-mechanical investigations. I will remark, beforehand, that the general law which I will lay before you answers the question in the negative.

By a similar plan, however, a speculative American set some time ago the industrial world of Europe in excitement. The magneto-electric machines often made use of in the case of rheumatic disorders are well known to the public. By imparting a swift rotation to the magnet of such a machine, we obtain powerful currents of electricity. If those be conducted through water, the latter will be reduced into its two components, oxygen and hydrogen. By the combustion of hydrogen, water is again generated. If this combustion takes place, not in atmospheric air, of which oxygen only constitutes a fifth part, but in pure oxygen, and if a bit of chalk be placed in the flame, the chalk will be raised to a white heat, and give us the sun-like Drummond's light. At the same time, the flame develops a considerable quantity of heat. Our American proposed to utilize in this way the gases obtained from electrolytic decomposition, and asserted that by the combustion a sufficient amount of heat was generated to keep a small steam engine in action, which again drove his magneto-electric machine, decomposed the water, and thus continually prepared its own fuel. This would certainly have been the most splendid of all discoveries; a perpetual motion which, besides the force which kept it going, generated light like the sun, and warmed all around it. The matter was by no means badly cogitated. Each practical step in the affair was known to be possible; but those who at that time were acquainted with the physical investigations which bear upon this subject could have affirmed, on first hearing the report, that the matter was to be numbered among

the numerous stories of the fable-rich America; and indeed a fable it remained.

It is not necessary to multiply examples further. You will infer from those given, in what immediate connection heat, electricity, magnetism, light, and chemical affinity, stand with mechanical forces.

Starting from each of these different manifestations of natural forces we can set every other in motion, for the most part not in one way merely, but in many ways. It is here as with the weaver's web—

Where a step stirs a thousand threads
The shuttles shoot from side to side,
The fibres flow unseen,
And one shock strikes a thousand combinations.

Now it is clear that if by any means we could succeed, as the above American professed to have done, by mechanical forces, to excite chemical, electrical, or other natural processes, which, by any circuit whatever, and without altering permanently the active masses in the machine, could produce mechanical force in greater quantity than that at first applied, a portion of the work thus gained might be made use of to keep the machine in motion, while the rest of the work might be applied to any other purpose whatever. The problem was, to find in the complicated net of reciprocal actions, a track through chemical, electrical, magnetical, and thermic processes, back to mechanical actions, which might be followed with a final gain of mechanical work; thus would the perpetual motion be found.

But, warned by the futility of former experiments, the public had become wiser. On the whole, people did not seek much after combinations which promised to furnish a perpetual motion, but the question was inverted. It was no more asked, how can I make use of the known and unknown relations of natural forces so as to construct a perpetual motion? but it was asked, if a perpetual motion be impossible, what are the relations which must subsist between natural forces? Everything was gained by this inversion of the question. The relations of natural forces rendered necessary by the above assumption, might be easily and completely stated. It was found that all known relations of force harmonize with the consequences of that assumption, and a series of unknown relations were discovered at

the same time, the correctness of which remained to be proved. If a single one of them could be proved false, then a perpetual motion would be possible.

The first who endeavoured to travel this way was a Frenchman, named Carnot, in the year 1824. In spite of a too limited conception of his subject, and an incorrect view as to the nature of heat, which led him to some erroneous conclusions, his experiment was not quite unsuccessful. He discovered a law which now bears his name, and to which I will return further on.

His labors remained for a long time without notice, and it was not till eighteen years afterwards, that is, in 1842, that different investigators in different countries, and independent of Carnot, laid hold of the same thought.

The first who saw truly the general law here referred to, and expressed it correctly, was a German physician, J. R. Mayer, of Heilbronn, in the year 1842. A little later, in 1843, a Dane, named Colding, presented a memoir to the Academy of Copenhagen, in which the same law found utterance, and some experiments were described for its further corroboration. In England, Joule began about the same time to make experiments having reference to the same subject. We often find, in the case of questions to the solution of which the development of science points, that several heads, quite independent of each other, generate exactly the same series of reflections.

I myself, without being acquainted with either Mayer or Colding, and having first made the acquaintance of Joule's experiments at the end of my investigation, followed the same path. I endeavoured to ascertain all the relations between the different natural processes, which followed from our regarding them from the above point of view. My inquiry was made public in 1847, in a small pamphlet bearing the title, "On the Conservation of Force."

Since that time the interest of the scientific public for this subject has gradually augmented. A great number of the essential consequences of the above manner of viewing the subject, the proof of which was wanting when the first theoretic notions were published, have since been confirmed by experiment, particularly by those of Joule; and during the last year the most eminent physicist of France, Regnault, has adopted the new mode regarding the question, and by fresh investigations on the specific heat of gases has contributed

much to its support. For some important consequences the experimental proof is still wanting, but the number of confirmations is so predominant, that I have not deemed it too early to bring the subject before even a non-scientific audience.

How the question has been decided you may already infer from what has been stated. In the series of natural processes there is no circuit to be found, by which mechanical force can be gained without a corresponding consumption. The perpetual motion remains impossible. Our reflections, however, gain thereby a higher interest.

We have thus far regarded the development of force by natural processes, only in its relation to its usefulness to man, as mechanical force. You now see that we have arrived at a general law, which holds good wholly independent of the application which man makes of natural forces; we must therefore make the expression of our new law correspond to this more general significance. It is in the first place clear, that the work which, by any natural process whatever, is performed under favourable conditions by a machine, and which may be measured in the way already indicated, may be used as a measure of force common to all. Further, the important question arises, "If the quantity of force cannot be augmented except by corresponding consumption, can it be diminished or lost?" For the purpose of our machines it certainly can, if we neglect the opportunity to convert natural processes to use, but as investigation has proved, not for a nature as a whole.

In the collision and friction of bodies against each other, the mechanics of former years assumed simply that living force was lost. But I have already stated that each collision and each act of friction generates heat; and, moreover, Joule has established by experiment the important law that for every foot-pound of force which is lost a definite quantity of heat is always generated, and that when work is performed by the consumption of heat, for each foot-pound thus gained a definite quantity of heat disappears. The quantity of heat necessary to raise the temperature of a pound of water a degree of the centigrade thermometer, corresponds to a mechanical force by which a pound weight would be raised to the height of 1350 feet; we name this quantity the mechanical equivalent of heat. I may mention here that these facts conduct of necessity to the conclusion, that the heat is not, as was formerly imagined, a fine imponderable substance,

but that, like light, it is a peculiar shivering motion of the ultimate particles of bodies. In collision and friction, according to this manner of viewing the subject, the motion of the mass of a body which is apparently lost is converted into a motion of the ultimate particles of the body; and conversely, when mechanical force is generated by heat, the motion of the ultimate particles is converted into a motion of the mass.

Chemical combinations generate heat, and the quantity of this heat is totally independent of the time and steps through which the combination has been effected, provided that other actions are not at the same time brought into play. If, however, mechanical work is at the same time accomplished, as in the case of the steam engine, we obtain as much less heat as is equivalent to this work. The quantity of work produced by chemical force is in general very great. A pound of the purest coal gives when burnt, sufficient heat to raise the temperature of 8086 pounds of water one degree of the centigrade thermometer; from this we can calculate that the magnitude of the chemical force of attraction between the particles of a pound of coal and the quantity of oxygen that corresponds to it is capable of lifting a weight of one hundred pounds to a height of twenty miles. Unfortunately, in our steam engines, we have hitherto been able to gain only the smallest portion of this work; the greater part is lost in the shape of heat. The best expansive engines give back as mechanical work only eighteen per cent. of the heat generated by the fuel.

From a similar investigation of all the other known physical and chemical processes, we arrive at the conclusion that Nature as a whole possesses a store of force which cannot in any way be either increased or diminished. And that, therefore, the quantity of force in Nature is just as eternal and unalterable as the quantity of matter. Expressed in this form, I have named the general law "The Principle of the Conservation of Force."

We cannot create mechanical force, but we may help ourselves from the general store-house of Nature. The brook and the wind, which drive our mills, the forest and the coal-bed, which supply our steam engines and warm our rooms, are to us the bearers of a small portion of the great natural supply which we draw upon for our purposes, and the actions of which we can apply as we think fit. The possessor of a mill claims the gravity of the descending rivulet, or

the living force of the moving wind, as his possession. These portions of the store of Nature are what give his property its chief value.

Further, from the fact that no portion of force can be absolutely lost, it does not follow that a portion may not be inapplicable to human purposes. In this respect the inferences drawn by William Thomson from the law of Carnot are of importance. This law, which was discovered by Carnot during his endeavours to ascertain the relations between heat and mechanical force, which, however, by no means belongs to the necessary consequences of the conservation of force, and which Clausius was the first to modify in such a manner that it no longer contradicted the above general law, expresses a certain relation between the compressibility, the capacity for heat, and the expansion by heat of all bodies. It is not yet considered as actually proved, but some remarkable deductions having been drawn from it, and afterwards proved to be facts by experiment, it has attained thereby a great degree of probability. Besides the mathematical form in which the law was first expressed by Carnot, we can give it the following more general expression:—"Only, when heat passes from a warmer to a colder body, and even then only partially, can it be converted into mechanical work."

The heat of a body which we cannot cool further, cannot be changed into another form of force; into the electric or chemical force, for example. Thus, in our steam engines, we convert a portion of the heat of the glowing coal into work, by permitting it to pass to the less warm water of the boiler. If, however, all the bodies in nature had the same temperature, it would be impossible to convert any portion of their heat into mechanical work. According to this, we can divide the total force store of the universe into two parts, one of which is heat, and must continue to be such; the other, to which a portion of the heat of the warmer bodies, and the total supply of chemical, mechanical, electrical, and magnetical forces belong, is capable of the most varied changes of form, and constitutes the whole wealth of change which takes place in nature.

But the heat of the warmer bodies strives perpetually to pass to bodies less warm by radiation and conduction, and thus to establish an equilibrium of temperature. At each motion of a terrestrial body, a portion of mechanical force passes by friction or collision into heat, of which only a part can be converted back again into mechanical

force. This is also generally the case in every electrical and chemical process. From this, it follows that the first portion of the store of force, the unchangeable heat, is augmented by every natural process, while the second portion, mechanical, electrical, and chemical force, must be diminished; so that if the universe be delivered over to the undisturbed action of its physical processes, all force will finally pass into the form of heat, and all heat come into a state of equilibrium. Then all possibility of a further change would be at an end, and the complete cessation of all natural processes must set in. The life of men, animals, and plants, could not of course continue if the sun had lost its high temperature, and with it his light,—if all the components of the earth's surface had closed those combinations which their affinities demand. In short, the universe from that time forward would be condemned to a state of eternal rest.

These consequences of the law of Carnot are, of course, only valid, provided that the law, when sufficiently tested, proves to be universally correct. In the mean time there is little prospect of the law being proved incorrect. At all events we must admire the sagacity of Thomson, who, in the letters of a long known little mathematical formula, which only speaks of the heat, volume, and pressure of bodies, was able to discern consequences which threatened the universe, though certainly after an infinite period of time, with eternal death.

I have already given you notice that our path lay through a thorny and unrefreshing field of mathematico-mechanical developments. We have now left this portion of our road behind us. The general principle which I have sought to lay before you has conducted us to a point from which our view is a wide one, and aided by this principle, we can now at pleasure regard this or the other side of the surrounding world, according as our interest in the matter leads us. A glance into the narrow laboratory of the physicist, with its small appliances and complicated abstractions, will not be so attractive as a glance at the wide heaven above us, the clouds, the rivers, the woods, and the living beings around us. While regarding the laws which have been deduced from the physical processes of terrestrial bodies, as applicable also to the heavenly bodies, let me remind you that the same force which, acting at the earth's surface, we call gravity (*Schwere*), acts as gravitation in the celestial spaces, and also manifests its power in the motion of the immeasurably distant double

stars which are governed by exactly the same laws as those subsisting between the earth and moon; that, therefore, the light and heat of terrestrial bodies do not in any way differ essentially from those of the sun, or of the most distant fixed star; that the meteoric stones which sometimes fall from external space upon the earth are composed of exactly the same simple chemical substances as those with which we are acquainted. We need, therefore, feel no scruple in granting that general laws to which all terrestrial natural processes are subject, are also valid for other bodies than the earth. We will, therefore, make use of our law to glance over the household of the universe with respect to the store of force, capable of action, which it possesses.

A number of singular peculiarities in the structure of our planetary system indicate that it was once a connected mass with a uniform motion of rotation. Without such an assumption, it is impossible to explain why all the planets move in the same direction round the sun, why they all rotate in the same direction round their axes, why the planes of their orbits, and those of their satellites and rings all nearly coincide, why all their orbits differ but little from circles; and much besides. From these remaining indications of a former state, astronomers have shaped an hypothesis regarding the formation of our planetary system, which, although from the nature of the case it must ever remain an hypothesis, still in its special traits is so well supported by analogy, that it certainly deserves our attention. It was Kant who, feeling great interest in the physical description of the earth and the planetary system, undertook the labour of studying the works of Newton, and as an evidence of the depth to which he had penetrated into the fundamental ideas of Newton, seized the notion that the same attractive force of all ponderable matter which now supports the motion of the planets, must also aforesaid have been able to form from matter loosely scattered in space the planetary system. Afterwards, and independent of Kant, Laplace, the great author of the *Mecanique Celeste*, laid hold of the same thought, and introduced it among astronomers.

The commencement of our planetary system, including the sun, must, according to this, be regarded as an immense nebulous mass which filled the portion of space which is now occupied by our system, far beyond the limits of Neptune, our most distant planet. Even

now we perhaps see similar masses in the distant regions of the firmament, as patches of nebulae, and nebulous stars; within our system also, comets, the zodiacal light, the corona of the sun during a total eclipse, exhibit remnants of a nebulous substance, which is so thin that the light of the stars passes through it unenfeebled and unrefracted. If we calculate the density of the mass of our planetary system, according to the above assumption, for the time when it was a nebulous sphere, which reached to the path of the outmost planet, we should find that it would require several cubic miles of such matter to weigh a single grain.

The general attractive force of all matter must, however, impel these masses to each other, and to condense, so that the nebulous sphere became incessantly smaller, by which, according to mechanical laws, a motion of rotation originally slow, and the existence of which must be assumed, would gradually become quicker and quicker. By the centrifugal force which must act most energetically in the neighbourhood of the equator of the nebulous sphere, masses could from time to time be torn away, which afterwards would continue their courses separate from the main mass, forming themselves into single planets, or, similar to the great original sphere, into planets with satellites and rings, until finally the principal mass condensed itself into the sun. With regard to the origin of heat and light, this view gives us no information.

When the nebulous chaos first separated itself from other fixed star masses, it must not only have contained all kinds of matter which was to constitute the future planetary system, but also, in accordance with our new law, the whole store of force which at one time must unfold therein its wealth of actions. Indeed in this respect an immense dower was bestowed in the shape of the general attraction of all the particles for each other. This force, which on the earth exerts itself as gravity, acts in the heavenly spaces as gravitation. As terrestrial gravity when it draws a weight downwards performs work and generates *vis viva*, so also the heavenly bodies do the same when they draw two portions of matter from distant regions of space towards each other.

The chemical forces must have been also present, ready to act; but as these forces can only come into operation by the most intimate

contact of the different masses, condensation must have taken place before the play of chemical forces began.

Whether a still further supply of force in the shape of heat was present at the commencement we do not know. At all events, by aid of the law of the equivalence of heat and work, we find in the mechanical forces, existing at the time to which we refer, such a rich source of heat and light, that there is no necessity whatever to take refuge in the idea of a store of these forces originally existing. When through condensation of the masses their particles came into collision, and clung to each other, the *vis viva* of their motion would be thereby annihilated, and must reappear as heat. Already in old theories, it has been calculated that cosmical masses must generate heat by their collision, but it was far from anybody's thought to make even a guess at the amount of heat to be generated in this way. At present we can give definite numerical values with certainty.

Let us make this addition to our assumption; that, at the commencement, the density of the nebulous matter was a vanishing quantity, as compared with the present density of the sun and planets; we can then calculate how much work has been performed by the condensation; we can further calculate how much of this work still exists in the form of mechanical force, as attraction of the planets towards the sun, and as *vis viva* of their motion, and find by this how much of the force has been converted into heat.

The result of this calculation is, that only about the 454th part of the original mechanical force remains as such, and that the remainder, converted into heat, would be sufficient to raise a mass of water equal to the sun and planets taken together, not less than twenty-eight millions of degrees of the centigrade scale. For the sake of comparison, I will mention that the highest temperature which we can produce by the oxyhydrogen blowpipe, which is sufficient to fuse and vaporize even platina, and which but few bodies can endure, is estimated at about two thousand centigrade degrees. Of the action of a temperature of twenty-eight millions of such degrees we can form no notion. If the mass of our entire system were pure coal, by the combustion of the whole of it only the 3500th part of the above quantity would be generated. This is also clear, that such a development of heat must have presented the greatest obstacle to the

speedy union of the masses, that the larger part of the heat must have been diffused by radiation into space, before the masses could form bodies possessing the present density of the sun and planets, and that these bodies must once have been in a state of fiery fluidity. This notion is corroborated by the geological phenomena of our planet; and with regard to the other planetary bodies, the flattened form of the sphere, which is the form of equilibrium of a fluid mass, is indicative of a former state of fluidity. If I thus permit an immense quantity of heat to disappear without compensation from our system, the principle of the conservation of force is not thereby invaded. Certainly for our planet it is lost, but not for the universe. It has proceeded outwards, and daily proceeds outwards into infinite space; and we know not whether the medium which transmits the undulations of light and heat possesses an end where the rays must return, or whether they eternally pursue their way through infinitude.

The store of force at present possessed by our system, is also equivalent to immense quantities of heat. If our earth were by a sudden shock brought to rest on her orbit—which is not to be feared in the existing arrangements of our system—by such a shock a quantity of heat would be generated equal to that produced by the combustion of fourteen such earths of solid coal. Making the most unfavourable assumption as to its capacity for heat, that is, placing it equal to that of water, the mass of the earth would thereby be heated 11,200 degrees; it would therefore be quite fused and for the most part reduced to vapour. If, then, the earth, after having been thus brought to rest, should fall into the sun, which of course would be the case, the quantity of heat developed by the shock would be four hundred times greater.

Even now, from time to time, such a process is repeated on a small scale. There can hardly be a doubt that meteors, fire-balls, and meteoric stones are masses which belong to the universe, and before coming into the domain of our earth, moved like the planets round the sun. Only when they enter our atmosphere do they become visible and fall sometimes to the earth. In order to explain the emission of light by these bodies, and the fact that for some time after their descent they are very hot, the friction was long ago thought of which they experience in passing through the air. We can now calculate that a velocity of 3,000 feet a second, supposing the whole of the fric-

tion to be expended in heating the solid mass, would raise a piece of meteoric iron $1,000^{\circ}$ C. in temperature, or, in other words, to a vivid red heat. Now the average velocity of the meteors seems to be thirty or forty times the above amount. To compensate this, however, the greater portion of the heat is, doubtless, carried away by the condensed mass of air which the meteor drives before it. It is known that bright meteors generally leave a luminous trail behind them, which probably consists of several portions of the red-hot surfaces. Meteoric masses which fall to the earth often burst with a violent explosion, which may be regarded as a result of the quick heating. The newly-fallen pieces have been for the most part found hot, but not red-hot, which is easily explainable by the circumstances, that during the short time occupied by the meteor in passing through the atmosphere, only a thin, superficial layer is heated to redness, while but a small quantity of heat has been able to penetrate to the interior of the mass. For this reason the red heat can speedily disappear.

Thus has the falling of the meteoric stone, the minute remnant of processes which seems to have played an important part in the formation of the heavenly bodies, conducted us to the present time, where we pass from the darkness of hypothetical views to the brightness of knowledge. In what we have said, however, all that is hypothetical is the assumption of Kant and Laplace, that the masses of our system were once distributed as *nebulae* in space.

On account of the rarity of the case, we will still further remark, in what close coincidence the results of science here stand with the earlier legends of the human family, and the forebodings of poetic fancy. The cosmogony of ancient nations generally commences with chaos and darkness.

Neither is the Mosaic tradition very divergent, particularly when we remember that that which Moses names heaven is different from the blue dome above us, and is synonymous with space, and that the unformed earth, and the waters of the great deep, which were afterwards divided into waters above the firmament, and waters below the firmament, resembled the chaotic components of the world.

Our earth bears still the unmistakable traces of its old fiery fluid condition. The granite formations of her mountains exhibit a structure, which can only be produced by the crystallization of fused masses. Investigation still shows that the temperature in mines, and borings,

increases as we descend; and if this increase is uniform, at the depth of fifty miles, a heat exists sufficient to fuse all our minerals. Even now our volcanoes project, from time to time, mighty masses of fused rocks from their interior, as a testimony of the heat which exists there. But the cooled crust of the earth has already become so thick, that, as may be shown by calculations of its conductive power, the heat coming to the surface from within, in comparison with that reaching the earth from the sun, is exceedingly small, and increases the temperature of the surface only about one-thirtieth of a degree centigrade; so that the remnant of the old store of force which is enclosed as heat within the bowels of the earth, has a sensible influence upon the processes at the earth's surface, only through the instrumentality of volcanic phenomena. These processes owe their power almost wholly to the action of other heavenly bodies, particularly to the light and heat of the sun, and partly also, in the case of the tides, to the attraction of the sun and moon.

Most varied and numerous are the changes which we owe to the light and heat of the sun. The sun heats our atmosphere irregularly, the warm rarefied air ascends, while fresh cool air flows from the sides to supply its place: in this way winds are generated. This action is most powerful at the equator, the warm air of which incessantly flows in the upper regions of the atmosphere towards the poles: while just as persistently, at the earth's surface, the trade wind carries new and cool air to the equator. Without the heat of the sun all winds must, of necessity, cease. Similar currents are produced by the same cause in the waters of the sea. Their power may be inferred from the influence which in some cases they exert upon climate. By them the warm water of the Antilles is carried to the British Isles, and confers upon them a mild, uniform warmth and rich moisture; while, through similar causes, the floating ice of the North Pole is carried to the coast of Newfoundland, and produces cold. Further, by the heat of the sun, a portion of the water is converted into vapour which rises in the atmosphere, is condensed to clouds, or falls in rain and snow upon the earth, collects in the form of springs, brooks, and rivers, and finally reaches the sea again, after having gnawed the rocks, carried away the light earth, and thus performed its part in the geologic changes of the earth; perhaps, besides all this it has driven our water-mill upon its way. If the heat of the sun were withdrawn,

there would remain only a single motion of water, namely, the tides, which are produced by the attraction of the sun and moon.

How is it now, with the motions and the work of organic beings? To the builders of the automata of the last century, men and animals appeared as clockwork which was never wound up, and created the force which they exerted out of nothing. They did not know how to establish a connection between the nutriment consumed and the work generated. Since, however, we have learned to discern in the steam-engine this origin of mechanical force, we must inquire whether something similar does not hold good with regard to men. Indeed, the continuation of life is dependent on the consumption of nutritive materials: these are combustible substances, which, after digestion and being passed into the blood, actually undergo a slow combustion, and finally enter into almost the same combinations with the oxygen of the atmosphere that are produced in an open fire. As the quantity of heat generated by combustion is independent of the duration of the combustion and the steps in which it occurs, we can calculate from the mass of the consumed material how much heat, or its equivalent work is thereby generated in an animal body. Unfortunately, the difficulty of the experiments is still very great; but within those limits of accuracy which have been as yet attainable, the experiments show that the heat generated in the animal body corresponds to the amount which would be generated by the chemical processes. The animal body therefore does not differ from the steam-engine, as regards the manner in which it obtains heat and force, but does differ from it in the manner in which the force gained is to be made use of. The body is, besides, more limited than the machine in the choice of its fuel; the latter could be heated with sugar, with starch-flour, and butter, just as well as with coal or wood; the animal body must dissolve its materials artificially, and distribute them through its system; it must, further, perpetually renew the used-up materials of its organs, and as it cannot itself create the matter necessary for this, the matter must come from without. Liebig was the first to point out these various uses of the consumed nutriment. As material for the perpetual renewal of the body, it seems that certain definite albuminous substances which appear in plants, and form the chief mass of the animal body, can alone be used. They form only a portion of the mass of nutriment taken daily; the remainder, sugar, starch,

fat, are really only materials for warming, and are perhaps not to be superseded by coal, simply because the latter does not permit itself to be dissolved.

If, then, the processes in the animal body are not in this respect to be distinguished from inorganic processes, the question arises, whence comes the nutriment which constitutes the source of the body's force? The answer is, from the vegetable kingdom; for only the material of plants, or the flesh of plant-eating animals, can be made use of for food. The animals which live on plants occupy a mean position between carnivorous animals, in which we reckon man, and vegetables, which the former could not make use of immediately as nutriment. In hay and grass the same nutritive substances are present as in meal and flour, but in less quantity. As, however, the digestive organs of man are not in a condition to extract the small quantity of the useful from the great excess of the insoluble, we submit, in the first place, these substances to the powerful digestion of the ox, permit the nourishment to store itself in the animal's body, in order in the end to gain it for ourselves in a more agreeable and useful form. In answer to our question, therefore, we are referred to the vegetable world. Now when what plants take in and what they give out are made the subjects of investigation, we find that the principal part of the former consists in the products of combustion which are generated by the animal. They take the consumed carbon given off in respiration, as carbonic acid, from the air, the consumed hydrogen as water, the nitrogen in its simplest and closest combinations as ammonia; and from these materials, with the assistance of small ingredients which they take from the soil, they generate anew the compound combustible substances, albumen, sugar, oil, on which the animal subsists. Here, therefore, is a circuit which appears to be a perpetual store of force. Plants prepare fuel and nutriment, animals consume these, burn them slowly in their lungs, and from the products of combustion the plants again derive their nutriment. The latter is an eternal source of chemical, the former of mechanical forces. Would not the combination of both organic kingdoms produce the perpetual motion? We must not conclude hastily: further inquiry shows, that plants are capable of producing combustible substances only when they are under the influence of the sun. A portion of the sun's rays exhibits a remarkable relation to chemical forces,—

it can produce and destroy chemical combinations; and these rays, which for the most part are blue or violet, are called therefore chemical rays. We make use of their action in the production of photographs. Here compounds of silver are decomposed at the place where the sun's rays strike them. The same rays overpower in the green leaves of plants the strong chemical affinity of the carbon of the carbonic acid for oxygen, give back the latter free to the atmosphere, and accumulate the other, in combination with other bodies, as woody fibre, starch, oil, or resin. These chemically active rays of the sun disappear completely as soon as they encounter the green portions of the plants, and hence it is that in daguerreotype images the green leaves of plants appear uniformly black. Inasmuch as the light coming from them does not contain the chemical rays, it is unable to act upon the silver compounds.

Hence a certain portion of force disappears from the sunlight, while combustible substances are generated and accumulated in plants; and we can assume it as very probable, that the former is the cause of the latter. I must indeed remark, that we are in possession of no experiments from which we might determine whether the *vis viva* of the sun's rays which have disappeared, corresponds to the chemical forces accumulated during the same time; and as long as these experiments are wanting, we cannot regard the stated relation as a certainty. If this view should prove correct, we derive from it the flattering result, that all force, by means of which our bodies live and move, finds its source in the purest sunlight; and hence we are all, in point of nobility, not behind the race of the great monarch of China, who heretofore alone called himself Son of the Sun. But it must also be conceded that our lower fellow-beings, the frog and leech, share the same ethereal origin, as also the whole vegetable world, and even the fuel which comes to us from the ages past, as well as the youngest offspring of the forest with which we heat our stoves and set our machines in motion.

You see, then, that the immense wealth of ever-changing meteorological, climatic, geological, and organic processes of our earth are almost wholly preserved in action by the light and heat-giving rays of the sun; and you see in this a remarkable example, how Proteus-like the effects of a single cause, under altered external conditions, may exhibit itself in nature. Besides these, the earth experiences an action

of another kind from its central luminary, as well as from its satellite the moon, which exhibits itself in the remarkable phenomenon of the ebb and flow of the tide.

Each of these bodies excites, by its attraction upon the waters of the sea, two gigantic waves, which flow in the same direction round the world, as the attracting bodies themselves apparently do. The two waves of the moon, on account of her greater nearness, are about three and a half times as large as those excited by the sun. One of these waves has its crest on the quarter of the earth's surface which is turned towards the moon, the other is at the opposite side. Both these quarters possess the flow of the tide, while the regions which lie between have the ebb. Although in the open sea the height of the tide amounts to only about three feet, and only in certain narrow channels, where the moving water is squeezed together, rises to thirty feet, the might of the phenomena is nevertheless manifest from the calculation of Bessel, according to which a quarter of the earth covered by the sea possesses, during the flow of the tide, about 25,000 cubic miles of water more than during the ebb, and that therefore such a mass of water must, in six and a quarter hours, flow from one quarter of the earth to the other.

The phenomena of the ebb and flow, as already recognized by Mayer, combined with the law of the conservation of force, stand in remarkable connection with the question of the stability of our planetary system. The mechanical theory of the planetary motions discovered by Newton teaches, that if a solid body in absolute *vacuo*, attracted by the sun, move around him in the same manner as the planets, this motion will endure unchanged through all eternity.

Now we have actually not only one, but several such planets, which move around the sun, and by their mutual attraction create little changes and disturbances in each other's paths. Nevertheless Laplace, in his great work, the *Mecanique Celeste*, has proved that in our planetary system all these disturbances increase and diminish periodically, and can never exceed certain limits, so that by this cause the external existence of the planetary system is unendangered.

But I have already named two assumptions which must be made: first, that the celestial spaces must be absolutely empty; and secondly, that the sun and planets must be solid bodies. The first is at least the

case as far as astronomical observations reach, for they have never been able to detect any retardation of the planets, such as would occur if they moved in a resisting medium. But on a body of less mass, the comet of Encke, changes are observed of such a nature: this comet describes ellipses round the sun which are becoming gradually smaller. If this kind of motion, which certainly corresponds to that through a resisting medium, be actually due to the existence of such a medium, a time will come when the comet will strike the sun; and a similar end threatens all the planets, although after a time, the length of which baffles our imagination to conceive of it. But even should the existence of a resisting medium appear doubtful to us, there is no doubt that the planets are not wholly composed of solid materials which are inseparably bound together. Signs of the existence of an atmosphere are observed on the Sun, on Venus, Mars, Jupiter, and Saturn. Signs of water and ice upon Mars; and our earth has undoubtedly a fluid portion on its surface, and perhaps a still greater portion of fluid within it. The motions of the tides, however, produce friction, all friction destroys *vis viva*, and the loss in this case can only affect the *vis viva* of the planetary system. We come thereby to the unavoidable conclusion, that every tide, although with infinite slowness, still with certainty, diminishes the store of mechanical force of the system; and as a consequence of this, the rotation of the planets in question round their axes must become more slow; they must therefore approach the sun, or their satellites must approach them. What length of time must pass before the length of our day is diminished one second by the action of the tide cannot be calculated, until the height and time of the tide in all portions of the ocean are known. This alteration, however, takes place with extreme slowness, as is known by the consequences which Laplace has deduced from the observations of Hipparchus, according to which, during a period of 2000 years, the duration of the day has not been shortened by the one-three-hundredth part of a second. The final consequence would be, but after millions of years, if in the mean time the ocean did not become frozen, that one side of the earth would be constantly turned towards the sun, and enjoy a perpetual day, whereas the opposite side would be involved in eternal night. Such a position we observe in our moon with regard to the earth, and also in the case of the satellites

as regards their planets; it is, perhaps, due to the action of the mighty ebb and flow to which these bodies, in the time of their fiery fluid condition, were subjected.

I would not have brought forward these conclusions, which again plunge us in the most distant future, if they were not unavoidable. Physico-mechanical laws are, as it were, the telescopes of our spiritual eye, which can penetrate into the deepest night of time, past and to come.

Another essential question as regards the future of our planetary system has reference to its future temperature and illumination. As the internal heat of the earth has but little influence on the temperature of the surface, the heat of the sun is the only thing which essentially affects the question. The quantity of heat falling from the sun during a given time upon a given portion of the earth's surface may be measured, and from this it can be calculated how much heat in a given time is sent out from the entire sun. Such measurements have been made by the French physicist Pouillet, and it has been found that the sun gives out a quantity of heat per hour equal to that which a layer of the densest coal ten feet thick would give out by its combustion; and hence in a year a quantity equal to the combustion of a layer of seventeen miles. If this heat were drawn uniformly from the entire mass of the sun, its temperature would only be diminished thereby one and one-third of a degree centigrade per year, assuming its capacity for heat to be equal to that of water. These results can give us an idea of the magnitude of the emission, in relation to the surface and mass of the sun; but they cannot inform us whether the sun radiates heat as a glowing body, which since its formation has its heat accumulated within it, or whether a new generation of heat by chemical processes takes place at the sun's surface. At all events the law of the conservation of force teaches us that no process analogous to those known at the surface of the earth, can supply for eternity an inexhaustible amount of light and heat to the sun. But the same law also teaches that the store of force at present existing, as heat, or as what may become heat, is sufficient for an immeasurable time. With regard to the store of chemical force in the sun, we can form no conjecture, and the store of heat there existing can only be determined by very uncertain estimations. If, however, we adopt the very probable view, that the

remarkably small density of so large a body is caused by its high temperature, and may become greater in time, it may be calculated that if the diameter of the sun were diminished only the ten-thousandth part of its present length, by this act a sufficient quantity of heat would be generated to cover the total emission for 2100 years. Such a small change besides it would be difficult to detect even by the finest astronomical observations.

Indeed, from the commencement of the period during which we possess historic accounts, that is, for a period of about 4000 years, the temperature of the earth has not sensibly diminished. From these old ages we have certainly no thermometric observations, but we have information regarding the distribution of certain cultivated plants, the vine, the olive tree, which are very sensitive to changes of the mean annual temperature, and we find that these plants at the present moment have the same limits of distribution that they had in the times of Abraham and Homer; from which we may infer backwards the constancy of the climate.

In opposition to this it has been urged, that here in Prussia the German knights in former times cultivated the vine, cellared their own wine and drank it, which is no longer possible. From this the conclusion has been drawn, that the heat of our climate has diminished since the time referred to. Against this, however, Dove has cited the reports of ancient chroniclers, according to which, in some peculiarly hot years, the Prussian grape possessed somewhat less than its usual quantity of acid. The fact also speaks not so much for the climate of the country as for the throats of the German drinkers.

But even though the force store of our planetary system is so immensely great, that by the incessant emission which has occurred during the period of human history it has not been sensibly diminished, even though the length of the time which must flow by, before a sensible change in the state of our planetary system occurs, is totally incapable of measurement, still the inexorable laws of mechanics indicate that this store of force, which can only suffer loss and not gain, must be finally exhausted. Shall we terrify ourselves by this thought? Men are in the habit of measuring the greatness and the wisdom of the universe by the duration and the profit which it promises to their own race; but the past history of the earth already shows what an insignificant moment the duration of the existence of our race upon

it constitutes. A Nineveh vessel, a Roman sword awakes in us the conception of grey antiquity. What the museums of Europe show us of the remains of Egypt and Assyria we gaze upon with silent astonishment, and despair of being able to carry our thoughts back to a period so remote. Still must the human race have existed for ages, and multiplied itself before the pyramids of Nineveh could have been erected. We estimate the duration of human history at 6000 years; but immeasurable as this time may appear to us, what is it in comparison with the time during which the earth carried successive series of rank plants and mighty animals, and no men; during which in our neighbourhood the amber-tree bloomed, and dropped its costly gum on the earth and in the sea; when in Siberia, Europe and North America groves of tropical palms flourished; where gigantic lizards, and after them elephants, whose mighty remains we still find buried in the earth, found a home? Different geologists, proceeding from different premises, have sought to estimate the duration of the above creative period, and vary from a million to nine million years. And the time during which the earth generated organic beings is again small when we compare it with the ages during which the world was a ball of fused rocks. For the duration of its cooling from 2000° to 200° centigrade, the experiments of Bishop upon basalt show that about 350 millions of years would be necessary. And with regard to the time during which the first nebulous mass condensed into our planetary system, our most daring conjectures must cease. The history of man, therefore, is but a short ripple in the ocean of time. For a much longer series of years than that during which man has already occupied this world, the existence of the present state of inorganic nature favourable to the duration of man seems to be secured, so that for ourselves and for long generations after us, we have nothing to fear. But the same forces of air and water, and of the volcanic interior, which produced former geological revolutions, and buried one series of living forms after another, act still upon the earth's crust. They more probably will bring about the last day of the human race than those distant cosmical alterations of which we have spoken, and perhaps force us to make way for new and more complete living forms, as the lizards and the mammoth have given place to us and our fellow-creatures which now exist.

Thus the thread which was spun in darkness by those who sought a

perpetual motion has conducted us to a universal law of nature, which radiates light into the distant nights of the beginning and of the end of the history of the universe. To our own race it permits a long but not an endless existence; it threatens it with a day of judgment, the dawn of which is still happily obscured. As each of us singly must endure the thought of his death, the race must endure the same. But above the forms of life gone by, the human race has higher moral problems before it, the bearer of which it is, and in the completion of which it fulfils its destiny.

XXXII

LOUIS PASTEUR

1822-1895

Louis Pasteur was born at Dôle, France, December 27, 1822, the son of a tanner. Educated at Arbois, Besançon, and the École Normale, he was appointed assistant professor of chemistry at the last-named institution. His first important work was in demonstrating the asymmetry of molecules. In 1863 he investigated fermentation and showed that it was caused by the growth of bacteria and later proved that it was also the cause of putrefaction, a suggestion which Lister employed in developing antiseptic surgery. In 1865 Pasteur discovered the bacillus which caused the silkworm disease. Taking up the principle of inoculation he applied it to smallpox and later extended it to other infectious diseases. He died September 28, 1895.

INOCULATION FOR HYDROPHOBIA *

Gentlemen:—Your Congress meetings are the place for the discussion of the gravest problems of medicine; they serve also to point out the great landmarks of the future. Three years ago, on the eve of the London Congress, the doctrine of micro-organisms, the ætiological cause of transmissible maladies, was still the subject of sharp criticisms. Certain refractory minds continued to uphold the idea that “disease is in us, from us, by us.”

It was expected that the decided supporters of the theory of the spontaneity of diseases would make a bold stand in London; but no opposition was made to the doctrine of “exteriority,” or external causes, the first cause of contagious diseases, and those questions were not discussed at all.

* From Address delivered August 10, 1884 at the Copenhagen meeting of the International Medical Congress.)

It was there seen, once again, that when all is ready for the final triumph of truth, the united conscience of a great assembly feels it instinctively and recognises it.

All clear-sighted minds had already foreseen that the theory of the spontaneity of diseases received its death-blow on the day when it became possible reasonably to consider the spontaneous generation of microscopic organisms as a myth, and when, on the other hand, the life-activity of those same beings was shown to be the main cause of organic decomposition and of all fermentation.

From the London Congress, also, dates the recognition of another very hopeful progress; we refer to the attenuation of different viruses, to the production of varying degrees of virulence for each virus, and their preservation by suitable methods of cultivation; to the practical application, finally, of those new facts in animal medicine.

New microbic prophylactic viruses have been added to those of fowl-cholera and of splenic fever. The animals saved from death by contagious diseases are now counted by hundreds of thousands, and the sharp opposition which those scientific novelties met with at the beginning was soon swept away by the rapidity of their onward progress.

Will the circle of practical applications of those new notions be limited in future to the prophylaxis of animal distempers? We must never think little of a new discovery, nor despair of its fecundity; but more than that, in the present instance, it may be asserted that the question is already solved in principle. Thus, splenic fever is common to animals and man, and we make bold to declare that, were it necessary to do so, nothing could be easier than to render man also proof against that affection. The process which is employed for animals might, almost without a change, be applied to him also. It would simply become advisable to act with an amount of prudence which the value of the life of an ox or a sheep does not call for. Thus, we should use three or four vaccine-viruses instead of two, of progressive intensity of virulence, and choose the first ones so weak that the patient should never be exposed to the slightest morbid complication, however susceptible to the disease he might be by his constitution.

The difficulty, then, in the case of human diseases, does not lie in the application of the new method of prophylaxis, but rather in the

knowledge of the physiological properties of their viruses. All our experiments must tend to discover the proper degree of attenuation for each virus. But experimentation, if allowable on animals, is criminal on man. Such is the principal cause of the complication of researches bearing on diseases exclusively human. Let us keep in mind, nevertheless, that the studies of which we are speaking were born yesterday only, that they have already yielded valuable results, and that new ones may be fairly expected when we shall have gone deeper into the knowledge of animal maladies, and of those in particular which affect animals in common with man.

The desire to penetrate farther forward in that double study led me to choose rabies as the subject of my researches, in spite of the darkness in which it was veiled.

The study of rabies was begun in my laboratory four years ago, and pursued since then without other interruption than what was inherent to the nature of the researches themselves, which present certain unfavourable conditions. The incubation of the disease is always protracted, the space disposed of is never sufficient, and it thus becomes impossible at a given moment to multiply the experiments as one would like. Notwithstanding those material obstacles, lessened by the interest taken by the French Government in all questions of great scientific interest, we now no longer count the experiments which we have made, my fellow workers and myself. I shall limit myself to-day to an exposition of our latest acquisitions.

The name alone of a disease, and of rabies above all others, at once suggests to the mind the notion of a remedy.

But it will, in the majority of cases, be labour lost to aim in the first instance at discovering a mode of cure. It is, in a manner, leaving all progress to chance. Far better to endeavour to acquaint oneself, first of all, with the nature, the cause, and the evolution of the disease, with a glimmering hope, perhaps, of finally arriving at its prophylaxis.

To this last method we are indebted for the result that rabies is no longer to-day to be considered as an insoluble riddle.

We have found that the virus of rabies develops itself invariably in the nervous system, brain, and spinal cord, in the nerves, and in the salivary glands; but it is not present at the same moment in every one of those parts. It may, for example, develop itself at the lower ex-

tremity of the spinal cord, and only after a time reach the brain. It may be met with at one or at several points of the encephalon whilst being absent at certain other points of the same region.

If an animal is killed whilst in the power of rabies, it may require a pretty long search to discover the presence here or there in the nervous system, or in the glands, of the virus of rabies. We have been fortunate enough to ascertain that in all cases, when death has been allowed to supervene naturally, the swelled-out portion, or bulb, of the medulla oblongata nearest to the brain, and uniting the spinal cord with it, is always rabid. When an animal has died of rabies (and the disease always ends in death), rabid matter can with certainty be obtained from its bulb, capable of reproducing the disease in other animals when inoculated into them, after trephining, in the arachnoid space of the cerebral meninges.

Any street dog whatsoever, inoculated in the manner described with portions of the bulb of an animal which has died of rabies, will certainly develop the same disease. We have thus inoculated several hundreds of dogs brought without any choice from the pound. Never once was the inoculation a failure. Similarly also, with uniform success, several hundred guinea-pigs, and rabbits more numerous still.

Those two great results, the constant presence of the virus in the bulb at the time of death, and the certainty of the reproduction of the disease by inoculation into the arachnoid space, stand out like experimental axioms, and their importance is paramount. Thanks to the precision of their application, and to the well-known daily repetition of those two criteria of our experiments, we have been able to move forward steadily and surely in that arduous study. But, however solid those experimental bases, they were, nevertheless, incapable in themselves of giving us the faintest notion as to some method of vaccination against rabies. In the present state of science the discovery of a method of vaccination against some virulent malady presupposes:

1. That we have to deal with a virus capable of assuming diverse intensities, of which the weaker ones can be put to vaccinal or protective uses.

2. That we are in possession of a method enabling us to reproduce those diverse degrees of virulence at will.

At the present time, however, science is acquainted with one sort of rabies only—viz., dog rabies.

Rabies, whether in dog, man, horse, ox, wolf, fox, etc., comes originally from the bite of a mad dog. It is never spontaneous, neither in the dog nor in any other animal. There are none seriously authenticated among the alleged cases of so-called spontaneous rabies, and I add that it is idle to argue that the first case of rabies of all must have been spontaneous. Such an argument does not solve the difficulty, and wantonly calls into question the as yet inscrutable problem of the origin of life. It would be quite as well, against the assertion that an oak tree always proceeded from another oak tree, to argue that the first of all oak trees that ever grew must have been produced spontaneously. Science, which knows itself, is well aware that it would be useless for her to discuss about the origin of things; she is aware that, for the present at any rate, that origin is placed beyond the ken of her investigations.

In fine, then, the first question to be solved on our way towards the prophylaxis of rabies is that of knowing whether the virus of that malady is susceptible of taking on varying intensities, after the manner of the virus of fowl-cholera or of splenic fever.

But in what way shall we ascertain the possible existence of varying intensities in the virus of rabies? By what standard shall we measure the strength of a virus which either fails completely or kills? Shall we have recourse to the visible symptoms of rabies? But those symptoms are extremely variable, and depend essentially on the particular point of the encephalon or of the spinal cord where the virus has in the first instance fixed and developed itself. The most caressing rabies, for such do exist, when inoculated into another animal of the same species, give rise to furious rabies of the intensest type.

Might we then perhaps make use of the duration of incubation as a means of estimating the intensity of our virus? But what can be more changeful than the incubative period? Suppose a mad dog were to bite several sound dogs: one of them will take rabies in one month or six weeks, another after two or three months or more. Nothing, too, is more changeful than the length of incubation according to the different modes of inoculation. Thus, other circumstances the same, after bites or hypodermic inoculation rabies occasionally develops itself, and at other times aborts completely; but inoculations on the brain are never sterile, and give the disease after a relatively short incubation.

It is possible, nevertheless, to gauge with sufficient accuracy the degree of intensity of our virus by means of the time of incubation, on condition that we make use exclusively of the intra-cranial mode of inoculation; and secondly, that we do away with one of the great disturbing influences inherent to the results of inoculation made by bites, under the skin or in the veins, by injecting the right proportion of material.

The duration of incubation, as a matter of fact, may depend largely on the quantity of efficient virus—that is to say, on the quantity of virus which reaches the nervous system without diminution or modification. Although the quantity of virus capable of giving rabies may be, so to speak, infinitely small, as seen in the common fact of the disease developing itself after rabid bites which, as a rule, introduce into the system a barely appreciable weight of virus, it is easy to double the length of incubation by simply changing the proportion of those very small quantities of inoculated matter. I may quote the following examples:—

On May 10, 1882, we injected into the popliteal vein of a dog ten drops of a liquid prepared by crushing a portion of the bulb of a dog, which had died of ordinary canine madness, in three or four times its volume of sterilised broth.

Into a second dog we injected $\frac{1}{100}$ th of that quantity, into a third $\frac{1}{200}$ th. Rabies showed itself in the first dog on the eighteenth day after the injection, on the thirty-fifth day in the second dog, whilst the third one did not take the disease at all, which means that, for the last animal, with the particular mode of inoculation employed, the quantity of virus injected was not sufficient to give rabies. And yet that dog, like all dogs, was susceptible of taking the disease, for it actually took it twenty-two days after a second inoculation, performed on September 3, 1882.

I now take another example bearing on rabbits, and by a different mode of inoculation. This time, after trephining, the bulb of a rabbit which had died of rabies after inoculation of an extremely powerful virus is triturated and mixed with two or three times its volume of sterilised broth. The mixture is allowed to stand a little, and then two drops of the supernatant liquid are injected after trephining into a first rabbit, into a second rabbit one-fourth of that quantity, and in succession into other rabbits, $\frac{1}{16}$ th, $\frac{1}{64}$ th, $\frac{1}{128}$ th, and $\frac{1}{512}$ nd

of that same quantity. All those rabbits died of rabies, the incubation having been eight days, nine and ten days for the third and fourth, twelve and sixteen days for the last ones.

Those variations in the length of incubation were not the result of any weakening or diminution of the intrinsic virulence of the virus brought on possibly by its dilution, for the incubation of eight days was at once recovered when the nervous matter of all those rabbits was inoculated into new animals.

Those examples show that, whenever rabies follows upon bites or hypodermic inoculations, the differences in respect of length of incubation must be chiefly ascribed to the variations, at times within considerable limits, of the ever-undeterminate proportions of the inoculated viruses which reach the central nervous system.

If, therefore, we desire to make use of the length of incubation as a measure of the intensity of the virulence, it will be indispensable to have recourse to inoculation on the surface of the brain, after trephining, a process the action of which is absolutely certain, coupled with the use of a larger quantity of virus than what is strictly sufficient to give rise to rabies. By those means the irregularities in the length of incubation for the same virus tend to disappear completely, because we always have the maximum effect which that virus can produce; that maximum coincides with a minimum length of incubation.

We have thus, finally, become possessed of a method enabling us to investigate the possible existence of different degrees of virulence, and to compare them with one another. The whole secret of the method, I repeat, consists in inoculating on the brain, after trephining, a quantity of virus which, although small in itself, is still greater than what is simply necessary to reproduce rabies. We thus disengage the incubation from all disturbing influences and render its duration dependent exclusively on the activity of the particular virus used, that activity being in each case estimated by the minimum incubation determined by it.

This method was applied in the first instance to the study of canine madness, and in particular to the question of knowing whether dog-madness was always one and the same, with perhaps the slight variations which might be due to the differences of race in diverse dogs.

We accordingly got hold of a number of dogs affected with ordinary street rabies, at all times of the year, at all seasons of the same

year or of different years, and belonging to the most dissimilar canine races. In each case the bulbar portion of the medulla oblongata was taken out from the recently dead animal, triturated and suspended in two or three times its volume of sterilised liquid, making use all along of every precaution to keep our materials pure, and two drops of this liquid injected after trephining into one or two rabbits. The inoculation is made with a Pravaz syringe, the needle of which, slightly curved at its extremity, is inserted through the dura-mater into the arachnoid space. The results were as follows: all the rabbits, from whatever sort of dog inoculated, showed a period of incubation which ranged between twelve and fifteen days, without almost a single exception. Never did they show an incubation of eleven, ten, nine, or eight days, never an incubation of several weeks or of several months.

Dog-rabies, the ordinary rabies, the only known rabies, is thus sensibly one in its virulence, and its modifications, which are very limited, appear to depend solely on the varying aptitude for rabies of the different known races. But we are going now to witness a deep change in the virulence of dog-rabies.

Let us take one, any one, of our numerous rabbits, inoculated with the virus of an ordinary mad dog, and, after it has died, extract its bulb, prepare it just as described, and inject two drops of the bulb-emulsion into the arachnoid space of a second rabbit, whose bulb will in turn and in time be injected into a third rabbit, the bulb of which again will serve for a fourth rabbit, and so on.

There will be evidence, even from the first few passages, of a marked tendency towards a lessening of the period of incubation in the succeeding rabbits. Just one example:

Towards the end of the year 1882 fifteen cows and one bull died of rabies on a farm situated in the neighbourhood of the town of Melun. They had been bitten on October 2 by the farm dog, which had become mad. The head of one of the cows, which had died on November 15, was sent to my laboratory by M. Rossignol, a veterinary surgeon in Melun. A number of experiments were made on dogs and rabbits, and showed that the following parts, the only encephalic (or those pertaining to the brain) ones tested, were rabid: the bulb, the cerebellum, the frontal lobe, the sphenoidal lobe. The rabbits trephined and inoculated with those different parts showed the first symptoms of rabies on the seventeenth and eighteenth days after

inoculation. With the bulb of one of those rabbits two more were inoculated, of which one took rabies on the fifteenth day, the other on the twenty-third day.

We may notice, once for all, that when rabies is transferred from one animal to another of a different species, the period of incubation is always very irregular at first in the individuals of the second species if the virus had not yet become fixed in its maximum virulence for the first species. We have just seen an example of that phenomenon, since one of the rabbits had an incubation of fifteen days, the other of twenty-three days, both having received the same virus and all other circumstances remaining apparently the same for them.

The bulb of the first one of those last rabbits which died was injected into two more rabbits, still after trephining. One of them took rabies on the tenth day, the other on the fourteenth day. The bulb of the first one that died was again injected into a couple of new rabbits, which developed the disease in ten days and twelve days respectively. A fifth time two new animals were inoculated from the first one that died, and they both took the disease on the eleventh day after inoculation: similarly, a sixth passage was made, and gave an incubation of eleven days, twelve days for the seventh passage, ten and eleven for the eighth, ten days for the ninth and tenth passages, nine days for the eleventh, eight and nine days for the twelfth, and so on, with differences of twenty-four hours at the most, until we got to the twenty-first passage, when rabies declared itself in eight days, and subsequently to that always in eight days up to the fiftieth passage, which was only effected a few days ago. That long experimental series which is still going on was begun on November 15, 1882, and will be kept up for the purpose of preserving in our rabies virus that maximum virulence which it has come to now for some considerable time, as it is easy to calculate.

Allow me to call your attention to the ease and safety of the operations for trephining and then inoculating the virus. Throughout the last twenty months we have been able without a single interruption in the course of the series to carry the one initial virus through a succession of rabbits which were all trephined and inoculated every twelfth day or so.

Guinea-pigs reach more rapidly the maximum virulence of which they are susceptible. The period of incubation is in them also variable

and irregular at the beginning of the series of successive passages, but it soon enough fixes itself at a minimum of five days. The maximum virulence in guinea-pigs is reached after seven or eight passages only. It is worth noting that the number of passages required before reaching the maximum virulence, both in guinea-pigs and in rabbits, varies with the origin of the first virus with which the series is begun.

If now this rabies with maximum virulence be transferred again into the dog from guinea-pig or rabbit, there is produced a dog-virus which in point of virulence goes far beyond that of ordinary canine madness.

But, a natural query—of what use can be that discovery as to the existence and artificial production of diverse varieties of rabies, every one of them more violent and more rapidly fatal than the habitual madness of the dog? The man of science is thankful for the smallest find he can make in the field of pure science, but the many, terrified at the very name of hydrophobia, claim something more than mere scientific curiosities. How much more interesting it would be to become acquainted with a set of rabies viruses which should, on the contrary, be possessed of attenuated degrees of virulence! Then, indeed, might there be some hope of creating a number of vaccinal rabies viruses such as we have done for the virus of fowl-cholera, of the microbe of saliva, of the red evil of swine (swine-plague), and even of acute septicæmia. Unfortunately, however, the methods which had served for those different viruses showed themselves to be either inapplicable or inefficient in the case of rabies. It therefore became necessary to find out new and independent methods, such, for example, as the cultivation *in vitro* of the mortal rabies virus.

Jenner was the first to introduce into current science the opinion that the virus which he called the grease of the horse, and which we call now more exactly horse-pox, probably softened its virulence, so to speak, in passing through the cow and before it could be transferred to man without danger. It was therefore natural to think of a possible diminution of the virulence of rabies by a number of passages through the organisms of some animal or other, and the experiment was worth trying. A large number of attempts were made, but the majority of the animal species experimented on exalted the virulence after the manner of rabbits and guinea-pigs; fortunately, however, it was not so with monkey.

On December 6, 1883, a monkey was trephined and inoculated with the bulb of a dog, which had itself been similarly inoculated from a child who had died of rabies. The monkey took rabies eleven days later, and when dead served for inoculation into a second monkey, which also took the disease on the eleventh day. A third monkey, similarly inoculated from the second one, showed the first symptoms on the twenty-third day, etc. The bulb of each one of the monkeys was inoculated, after trephining, into two rabbits each time. The rabbits inoculated from the first monkey developed rabies between thirteen and sixteen days, those from the second monkey between fourteen and twenty days, those from the third monkey between twenty-six and thirty days, those from the fourth monkey both of them after the twenty-eighth day, those from the fifth monkey after twenty-seven days, those from the sixth monkey after thirty days.

It cannot be doubted after that, that successive passages through monkeys, and from the several monkeys to rabbits, do diminish the virulence of the virus for the latter animals; they diminish it for dogs also. The dog inoculated with the bulb of the fifth monkey gave an incubation of no less than fifty-eight days, although it had been inoculated in the arachnoid space.

The experiments were renewed with fresh sets of monkeys and led to similar results. We were therefore actually in possession of a method by means of which we could attenuate the virulence of rabies. Successive inoculations from monkey to monkey elaborate viruses which, when transferred to rabbits, reproduce rabies in them, but with a progressively lengthening period of incubation. Nevertheless, if one of those rabbits be taken as the first for inoculations through a series of rabbits, the rabies thus cultivated obeys the law which we have seen before, and has its virulence increased at each passage.

The practical application of those facts gives us a method for the vaccination of dogs against rabies. As a starting point, make use of one of the rabbits inoculated from a monkey sufficiently removed from the first animal of the monkey series for the inoculation—hypodermic or intra-venous—of that rabbit's bulb not to be mortal for a new rabbit. The next vaccinal inoculations are made with the bulbs of rabbits derived by successive passages from that first rabbit.

In the course of our experiments we made use, as a rule, for inoculation, of the virus of rabbits which had died after an incubation of

four weeks, repeating three or four times each the vaccinal inoculations made with the bulbs of rabbits derived in succession from one another and from the first one of the series, itself coming directly from the monkey. I abstain from giving more details, because certain experiments which are actually going on allow me to expect that the process will be greatly simplified.

You must be feeling, gentlemen, that there is a great blank in my communication; I do not speak of the micro-organism of rabies. We have not got it. The process for isolating it is still imperfect, and the difficulties of its cultivation outside the bodies of animals have not yet been got rid of, even by the use, as pabulum, of fresh nervous matter. The methods which we employed in our study of rabies ought all the more perhaps, on that account, to fix attention. Long still will the art of preventing diseases have to grapple with virulent maladies the micro-organic germs of which will escape our investigations. It is, therefore, a capital scientific fact that we should be able, after all, to discover the vaccination process for a virulent disease without yet having at our disposal its special virus and whilst yet ignorant of how to isolate or to cultivate its microbe.

As soon as the method for the vaccination of dogs was firmly established, and we had in our possession a large number of dogs which had been rendered refractory to rabies, I had the idea of submitting to a competent committee those of the facts which appeared destined in future to serve as a basis for the vaccination of dogs against rabies. That course was suggested to me in prevision of the later practical application of the method, by the recollection of the opposition with which Jenner's discovery met at its beginning.

I spoke of my project to M. Fallières, the Minister of Public Instruction, who was pleased to approve of it and gave commission to the following gentlemen to control the facts which I had summarily communicated to the Academy of Sciences in its sitting of May 19 last: Messrs. Béclard, Paul Bert, Bouley, Aimeraud, Villemin, Vulpian. M. Bouley was appointed president, Dr. Villemin, secretary, and the commission at once set to work. I have the pleasure of informing you that it has just sent in a first report to the Minister. I was acquainted with it here, and the following are in a few words, the facts related in that first report on rabies. I had given to the commission nineteen vaccinated dogs in succession—that is to say,

dogs which had been rendered refractory by preventive inoculations. Thirteen only of them had after their vaccination been already submitted to the test-inoculation on the brain.

The nineteen dogs were, for the sake of comparison, divided into sets along with nineteen more control dogs brought from the pound without any sort of selection. To begin with, two refractory dogs and two control dogs were on June 1 trephined and inoculated under the dura-mater, on the surface of the brain, with the bulb of a dog affected with ordinary street rabies.

On June 3 another refractory dog and another control dog were bitten by a furious street mad dog.

The same furious mad dog was on June 4 made to bite still another refractory and another control dog. On June 6 the furious dog which had been utilised on June 3 and 4 died. The bulb was taken out and inoculated, after trephining, into three refractory dogs and three control dogs. On June 10 another street mad dog, having been secured, was, by the commission, made to bite one refractory and one control dog. On June 16 the commission had two new dogs, a refractory one and a control one, bitten by one of the control dogs of June 1, which had been seized with rabies on June 14 in consequence of the inoculation after trephining which it had received on June 1.

On June 19 the commission got three refractory and three control dogs inoculated before their own eyes in the popliteal vein with the bulb of an ordinary street mad dog. On June 20 they had inoculated in their presence, and still in a vein, ten dogs altogether, six of them refractory and four just brought from the pound.

On June 28, the Commission hearing that M. Paul Simon, a veterinary surgeon, had a furious biting mad dog, had four of their dogs, two refractory and two control dogs, taken to his place and bitten by the mad dog.

The Rabies Commission have, therefore, experimented on thirty-eight dogs altogether—namely, nineteen refractory dogs and nineteen control dogs susceptible of taking the disease. Those of the dogs which have not died in consequence of the operations themselves are still under observation, and will long continue to be. The commission, reporting up to the present moment on their observations as to the state of the animals tried and tested by them, find that out of the nineteen control dogs six were bitten, of which six three have taken

rabies. Seven received intra-venous inoculations, of which five have died of rabies. Five were trephined and inoculated on the brain; the five have died of rabies.

On the other hand, not one of the nineteen vaccinated dogs has taken rabies.

In the course of the experiments, on July 13, one of the refractory dogs died in consequence of a black diarrhœa which had begun in the first days of July. In order to ascertain whether rabies had anything to do with it as the cause of death, its bulb was at once inoculated, after trephining, into three rabbits and one guinea-pig. All four animals are still to-day in perfect health, a certain proof that the dog died of some common malady, and not of rabies.

The second report of the Commission will be concerned with the experiments made as to the refractoriness to rabies of twenty dogs to be vaccinated by the Commission themselves.

(M. Pasteur then announced that he had just received that same morning the first report addressed to M. Fallières by the Official Commission on Rabies. It states that twenty-three refractory dogs were bitten by ordinary mad dogs, and that not one of them had taken rabies. On the other hand, within two months after the bites, 66 per cent. of the normal dogs similarly bitten had already taken the disease.)

November 1, 1886.—*New Communication on Rabies.*—On October 26, 1885, I acquainted the Academy with a method of prophylaxis of rabies after bites. Numerous applications on dogs had justified me in trying it on man. As early as March 1, 350 persons bitten by dogs undoubtedly mad, and several more by dogs simply suspected of rabies, had already been treated at my laboratory by Dr. Grancher. And in consideration of the happy results obtained it appeared to me that it had become necessary to found an establishment for anti-rabic vaccinations.

To-day, October 31, 1886, 2,490 persons have received the preventive inoculations in Paris alone. The treatment was in the first instance uniform for the great majority of the patients, notwithstanding the different conditions presented by them as to age, sex, the number of bites received, their seat, their depth, and the time which had elapsed since the occurrence of the accident. It lasted ten days,

the patient receiving every day an injection prepared from the spinal marrow of a rabbit, beginning with that of fourteen days' and ending with that of five days' desiccation.

Those 2,490 cases are subdivided according to nationality in the following manner:

Russia	191
Italy	165
Spain	107
England	80
Belgium	57
Austria	52
Portugal	25
Roumania	22
United States	18
Holland	14
Greece	10
Germany	9
Turkey	7
Brazil	3
India	2
Switzerland	2
France and Algeria	1,726

The number of French persons has been considerable, amounting to 1,726, and it will be enough to confine ourselves to the category formed by them as a basis for discussing the degree of efficacy of the method.

Out of the total 1,726 cases treated, the treatment has failed ten times—namely, in the following cases:

The children: Lagut, Peytel, Clédière, Moulis, Astier, Videau.

The woman: Leduc, seventy years old.

The men: Marius Bouvier (thirty years), Clergot (thirty), and Norbert Magnevon (eighteen).

I leave out of count two other persons, Louise Pelletier and Moermann, whose deaths must be attributed to their tardy arrival at the laboratory, Louise Pelletier thirty-six days, and Moermann forty-three days after they had been bitten.

We have therefore ten deaths for 1,726 cases, or 1 in 170; such

are, for France and Algeria, the results of the first year's application of the method.

Those statistics, taken as a whole, demonstrate the efficacy of the treatment, as proved further by the relatively large number of deaths which occurred amongst bitten persons who had not been vaccinated.

XXXIII

JAMES CLERK MAXWELL

1831-1879

James Clerk Maxwell, born November 13, 1831, attended Edinburgh University 1847-1850. Entering Cambridge, he graduated second wrangler in 1854. He then taught for four years in Marischal College, Aberdeen, and in 1860 was called to King's College, London, where he remained for the following eight years. He early revealed his mathematical genius and before he was nineteen had the honor of reading several papers before the Royal Society of Edinburgh. He developed by mathematics the theory that electricity was a condition of stress or strain in the ether, a wave moving in the same medium as light and traveling at the same rate of speed. The theory was substantiated by the experiments of Hertz, a pupil of Helmholtz, who in 1887 proved the existence of the waves which now bear his name. Maxwell died at Cambridge, November 5, 1879.

THE MAXWELL AND HERTZ THEORY OF ELECTRICITY AND LIGHT *

It was at the moment when the experiments of Fresnel were forcing the scientific world to admit that light consists of the vibrations of a highly attenuated fluid filling interplanetary spaces that the researches of Ampère were making known the laws of the mutual action of currents and were so enunciating the fundamental principles of electro-dynamics.

It needed but one step to the supposition that that same fluid, the ether, which is the medium of luminous phenomena, is at the same

* Translated from a paper by M. Henri Poincaré.

time the vehicle of electrical action. In imagination Ampère made this stride; but the illustrious physicist could not foresee that the seducing hypothesis with which he was toying, a mere dream for him, was ere long to take a precise form and become one of the vital concerns of exact science.

A dream it remained for many years, till one day, after electrical measurements had become extremely exact, some physicist, turning over the numerical data, much as a resting pedestrian might idly turn over a stone, brought to light an odd coincidence. It was that the factor of transformation between the system of electro-statical units and the system of electro-dynamical units was equal to the velocity of light. Soon the observations directed to this strange coincidence became so exact that no sane head could longer hold it a mere coincidence. No longer could it be doubted that some occult affinity existed between optical and electrical phenomena. Perhaps, however, we might be wondering to this day what this affinity could be were it not for the genius of Clerk Maxwell.

DISPLACEMENT CURRENTS

The reader is aware that solid bodies are divided into two classes, conductors through which electricity can move in the form of a galvanic current, and nonconductors, or dielectrics. The electricians of former days regarded dielectrics as quite inert, having no part to play but that of obstinately refusing passage to electricity. Had that been so, any one nonconductor might be replaced by any other without making any difference in the phenomena; but Faraday found that that was not the case. Two condensers of the same form and dimensions put into connection with the same source of electricity do not take the same charge, though the thickness of the isolating plate be the same, unless the matter of that plate be chemically the same. Now Clerk Maxwell had too deeply studied the researches of Faraday not to comprehend the importance of dielectrics and the imperative obligation to recognize their active part.

Besides, if light is but an electric phenomenon, when it traverses a thickness of glass electrical events must take place in that glass. And what can be the nature of those events? Maxwell boldly answers, they are, and must be, currents.

All the experience of his day seemed to contradict this. Never had

currents been observed except in conductors. How was Maxwell to reconcile his audacious hypothesis with a fact so well established as that? Why is it that under certain circumstances those supposed currents produce manifest effects, while under ordinary conditions they can not be observed at all?

The answer was that dielectrics resist the passage of electricity not so much more than conductors do, but in a different manner. Maxwell's idea will best be understood by a comparison.

If we bend a spring, we meet a resistance which increases the more the spring is bended. So, if we can only dispose of a finite force, a moment will come when the motion will cease, equilibrium being reached. Finally, when the force ceases the spring will in flying back restore the whole of the energy which has been expended in bending it.

Suppose, on the other hand, that we wish to displace a body plunged into water. Here again a resistance will be experienced, but it will not go on increasing in proportion as the body advances, supposing it to be maintained at a constant velocity. So long as the motive force acts, equilibrium will never, then, be attained; nor when the force is removed will the body in the least tend to return, nor can any portion of the energy expended be restored. It will, in fact, have been converted into heat by the viscosity of the water.

The contrast is plain; and we ought to distinguish elastic resistance from viscous resistance. Using these terms, we may express Maxwell's idea by saying that dielectrics offer an elastic resistance, conductors a viscous resistance, to the movements of electricity. Hence, there are two kinds of currents; currents of displacement which traverse dielectrics and ordinary currents of conduction which circulate in conductors.

Currents of the first kind, having to overcome an elastic resistance which continually increases, naturally can last but a very short time, since a state of equilibrium will quickly be reached.

Currents of conduction, on the other hand, having only a viscous resistance to overcome, must continue so long as there is any electromotive force.

Let us return to the simile used by M. Cornu in his notice in the *Annuaire du Bureau des Longitudes* for 1893. Suppose we have in a reservoir water under pressure. Lead a tube plumb downward in-

to the reservoir. The water will rise in the tube, but the rise will stop when hydrostatic equilibrium is attained—that is, when the downward pressure of the water in the tube above the point of application of the first pressure on the reservoir, and due to the weight of the water, balances that first pressure. If the pipe is large, there will be no friction or loss of head, and the water so raised can be used to do work. That represents a current of displacement.

If, on the other hand, the water flows out of the reservoir by a horizontal pipe, the motion will go on till the reservoir is emptied; but if the tube is small and long there will be a great loss of energy and considerable production of heat by friction. That represents a current of conduction.

Though it would be vain, not to say idle, to attempt to represent all details, it may be said that everything happens just as if the currents of displacement were acting to bend a multitude of little springs. When the currents cease, electrostatic equilibrium is established, and the springs are bent the more, the more intense is the electric field. The accumulated work of the springs—that is, the electrostatic energy—can be entirely restored as soon as they can unbend, and so it is that we obtain mechanical work when we leave the conductors to obey the electrostatic attractions. Those attractions must be due to the pressure exercised on the conductors by the bent springs. Finally, to pursue the image to the death, the disruptive discharge may be compared to the breaking of the springs when they are bent too much.

On the other hand, the energy employed to produce conduction currents is lost, being wholly converted into heat, like that spent in overcoming the viscosity of fluids. Hence it is that the conducting wires become heated.

From Maxwell's point of view it seems that all currents are in closed circuits. The older electricians did not so opine. They regarded the current circulating in a wire joining the two poles of a pile as closed; but if in place of directly uniting the two poles we place them in communication with the two armatures of a condenser, the momentary current which lasts while the condenser is getting charged was not considered as a current round a closed circuit. It went, they thought, from one armature through the wire, the battery, the other wire, to the other armature, and there it stopped. Maxwell, on the contrary, supposed that in the form of a current of displacement it passes through

the nonconducting plate of the condenser, and that precisely what brings it to cessation is the opposite electromotive force set up by the displacement of electricity in this dielectric.

Currents become sensible in three ways—by their heating effects, by their actions on other currents and on magnets, and by the induced currents to which they give rise. We have seen why currents of conduction develop heat and why currents of displacement do not. But Maxwell's hypothetical currents ought at any rate to produce electromagnetic and inductive effects. Why do these effects not appear? The answer is, that it is because a current of displacement can not last long enough. That is to say, they can not last long in one direction. Consequently in a dielectric no current can long exist without alteration. But the effects ought to and will become observable if the current is continually reversed at sufficiently short intervals.

THE NATURE OF LIGHT

Such, according to Maxwell, is the origin of light. A luminiferous wave is a series of alternating currents produced in dielectrics, in air, or even in the interplanetary void, and reversed in direction a million of million of times per second. The enormous induction due to these frequent alternations sets up other currents in the neighboring parts of the dielectric, and so the waves are propagated.

Calculation shows that the velocity of propagation would be equal to the ratio of the units, which we know is the velocity of light.

Those alternative currents are a sort of electrical oscillation. Are they longitudinal, like those of sound, or are they transversal, like those of Fresnal's ether? In the case of sound the air undergoes alternative condensations and rarefactions. The ether of Fresnal, on the other hand, behaves as if it were composed of incompressible layers capable only of slipping over one another. Were these currents in open paths, the electricity carried from one end to the other would become accumulated at one extremity. It would thus be condensed and rarefied like air, and its vibrations would be longitudinal. But Maxwell only admits currents in closed circuits; accumulation is impossible, and electricity behaves like

the incomprehensible ether of Fresnel, with its transversal vibrations.

EXPERIMENTAL VERIFICATION

We thus obtain all the results of the theory of waves. Yet this was not enough to decide the physicists to adopt the ideas of Maxwell. It was a seductive hypothesis; but physicists consider hypotheses which lead to no distinct observational consequences as beyond the borders of their province. That province, so defined, no experimental confirmation of Maxwell's theory invaded for twenty-five years.

What was wanted was some issue between the two theories not too delicate for our coarse methods of observation to decide. There was but one line of research along which any *experimentum crucis* was to be met with.

The old electro-dynamics makes electro-magnetic induction take place instantaneously; but according to Maxwell's doctrine it propagates itself with the velocity of light.

The point was then to measure, or at least to make certain, a velocity of propagation of inductive effects. This is what the illustrious German physicist Hertz has done by the method of interferences.

The method is well known in its application to optical phenomena. Two luminous rays from one identical center interfere when they reach the same point after pursuing paths of different lengths. If the difference is one, two, or any whole number of wave lengths, the two lights re-enforce one another so that if their intensities are equal, that of their combination is four times as great. But if the difference is an odd number of half wave lengths, the two lights extinguish one another.

Luminiferous waves are not peculiar in showing this phenomenon; it belongs to every periodic change which is propagated with definite velocity. Sound interferes just as light does, and so must electro-dynamic induction if it is strictly periodic and has a definite velocity of propagation. But if the propagation is instantaneous there can be no interference, since in that case there is no finite wave length.

The phenomenon, however, could not be observed were the wave length greater than the distance within which induction is sensible.

It is therefore requisite to make the period of alternation as short as possible.

ELECTRICAL EXCITERS

We can obtain such currents by means of an apparatus which constitutes a veritable electrical pendulum. Let two conductors be united by a wire. If they have not the same electric potential the electrical equilibrium is disturbed and tends to restore itself, just as the molar equilibrium is disturbed when a pendulum is carried away from the position of repose.

A current is set up in the wire, tending to equalize the potential, just as the pendulum begins to move so as to be carried back to the position of repose. But the pendulum does not stop when it reaches that position. Its inertia carries it farther. Nor, when the two electrical conductors reach the same potential, does the current in the wire cease. The equilibrium instantaneously existing is at once destroyed by a cause analogous to inertia, namely self-induction. We know that when a current is interrupted it gives rise in parallel wires to an induced current in the same direction. The same effect is produced in the circuit itself, if that is not broken. In other words, a current will persist after the cessation of its causes, just as a moving body does not stop the instant it is no longer driven forward.

When, then, the two potentials become equal, the current will go on and give the two conductors relative charges opposite to those they had at first. In this case, as in that of the pendulum, the position of equilibrium is passed, and a return motion is inevitable. Equilibrium, again instantaneously attained, is at once again broken for the same reason; and so the oscillations pursue one another unceasingly.

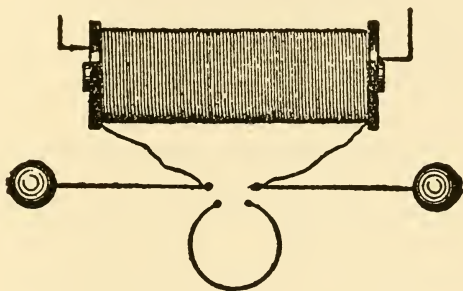
Calculation shows that the period depends on the capacity of the conductors in such a way that it is only necessary to diminish that capacity sufficiently (which is easily done) to have an electric pendulum capable of producing an alternating current of extremely short period.

All that was well enough known by the theoretical researches of Lord Kelvin and by the experimentation of Federson on the oscillatory discharge of the Leyden jar. It was not that which constituted the originality of Hertz.

But it is not enough to construct a pendulum ; it is further requisite to set it into oscillation. For that, it is necessary to carry it off from equilibrium and to let it go suddenly, that is to say, to release it in a time short as compared to the period of its oscillation.

For if, having pulled a pendulum to one side by a string, we were to let go of the string more slowly than the pendulum would have descended of itself, it would reach the vertical without momentum, and no oscillation would be set up.

In like manner, with an electric pendulum whose natural period is, say, a hundred-millionth of a second, no mechanical mode of release would answer the purpose at all, sudden as it might seem to us with our more than sluggish conceptions of promptitude. How, then, did Hertz solve the problem?



To return to our electric pendulum, a gap of a few millimeters is made in the wire which joins the two conductors. This gap divides our apparatus into two symmetrical parts, which are connected to the two poles of a Ruhmkorff coil. The induced current begins to charge the two conductors, and the difference of their potential increases with relative slowness.

At first the gap prevents a discharge from the conductors ; the air in it plays the rôle of insulator and maintains our pendulum in a position diverted from that of equilibrium.

But when the difference of potential becomes great enough, a spark will jump across. If the self-induction is great enough and the capacity and resistance small enough, there will be an oscillatory discharge whose period can be brought down to a hundred-millionth of a second.

The oscillatory discharge would not, it is true, last long by itself ; but it is kept up by the Ruhmkorff coil, whose current is itself oscillatory with a period of about a hundred-thousandth of a second, and thus the pendulum gets a new impulse as often as that.

The instrument just described is called a resonance exciter. It produces oscillations which are reversed from a hundred million to a thousand million times per second. Thanks to this extreme frequency, they can produce inductive effects at great distances. To make these effects sensible another electric pendulum is used, called a resonator. In this the coil is suppressed. It consists simply of two little metallic spheres very near to one another, with a long wire connecting them in a roundabout way.

The induction due to the exciter will set the resonator in vibration the more intensely the more nearly the natural periods of vibration are the same. At certain phases of the vibration the difference of potential of the two spheres will be just great enough to cause the sparks to leap across.

PRODUCTION OF THE INTERFERENCES

Thus we have an instrument which reveals the inductive waves which radiate from the exciter. We can study them in two ways. We may either expose the resonator to the direct induction of the exciter at a great distance, or else make this induction act at a small distance on a long conducting wire which the electric wave will follow and which in its turn will act at a small distance on the resonator.

Whether the wave is propagated along a wire or across the air, interferences can be produced by reflection. In the first case it will be reflected at the extremity of the wire, which it will again pass through in the opposite direction. In the second case it can be reflected on a metallic leaf which will act as a mirror. In either case the reflected ray will interfere with the direct ray, and positions will be found in which the spark of the resonator will be extinguished.

Experiments with a long wire are the easier and furnish much valuable information, but they cannot furnish an *experimentum crucis*, since in the old theory, as in the new, the velocity of the electric wave in a wire should be equal to that of light. But experiments on direct induction at great distances are decisive. They not only show that

the velocity of propagation of induction across air is finite, but also that it is equal to the velocity of the wave propagated along a wire, conformably to the ideas of Maxwell.

SYNTHESIS OF LIGHT

I shall insist less on other experiments of Hertz, more brilliant but less instructive. Concentrating with a parabolic mirror the wave of induction that emanates from the exciter, the German physicist obtained a true pencil of rays of electric force, susceptible of regular reflection and refraction. These rays, were the period but one-millionth of what it is, would not differ from rays of light. We know that the sun sends us several varieties of radiations, some luminiferous, since they act on the retina, others dark, infra-red, or ultra-violet, which reveal themselves in chemical and calorific effects. The first owe the qualities which render them sensible to us to a physiological chance. For the physicist, the infra-red differs from red only as red differs from green; it simply has a greater wave length. That of the Hertzian radiations is far greater still, but they are mere differences of degree, and if the ideas of Clerk Maxwell are true, the illustrious professor of Bonn has effected a genuine synthesis of light.

CONCLUSION

Nevertheless, our admiration for such unhopèd-for successes must not let us forget what remains to be accomplished. Let us endeavor to take exact account of the results definitely acquired.

In the first place, the velocity of direct induction through air is finite; for otherwise interferences could not exist. Thus the old electro-dynamics is condemned. But what is to be set up in its place? Is it to be the doctrine of Maxwell, or rather some approximation to that, for it would be too much to suppose that he had foreseen the truth in all its details? Though the probabilities are accumulating, no complete demonstration of that doctrine has ever attained.

We can measure the wave length of the Hertzian oscillations. That length is the product of the period into the velocity of propagation. We should know the velocity if we knew the period; but this last is so minute that we cannot measure it; we can only calculate it by a

formula due to Lord Kelvin. That calculation leads to figures agreeable to the theory of Maxwell; but the last doubts will only be dissipated when the velocity of propagation has been directly measured. (See Note I.)

But this is not all. Matters are far from being as simple as this brief account of the matter would lead one to think. There are various complications.

In the first place, there is around the exciter a true radiation of induction. The energy of the apparatus radiates abroad, and if no source feeds it, it quickly dissipates itself and the oscillations are rapidly extinguished. Hence arises the phenomenon of multiple resonance, discovered by Messrs. Sarasin and De la Rive, which at first seemed irreconcilable with the theory.

On the other hand, we know that light does not exactly follow the laws of geometrical optics, and the discrepancy, due to diffraction, increases proportionately to the wave length. With the great waves of the Hertzian undulations these phenomena must assume enormous importance and derange everything. It is doubtless fortunate, for the moment at least, that our means of observation are as coarse as they are, for otherwise the simplicity which struck us would give place to a dedalian complexity in which we should lose our way. No doubt a good many perplexing anomalies have been due to this. For the same reason the experiments to prove a refraction of the electrical waves can hardly be considered as demonstrative.

It remains to speak of a difficulty still more grave, though doubtless not insurmountable. According to Maxwell, the coefficient of electrostatic induction of a transparent body ought to be equal to the square of its index of refraction. Now this is not so. The few bodies which follow Maxwell's law are exceptions. The phenomena are plainly far more complex than was at first thought. But we have not yet been able to make out how matters stand, and the experiments conflict with one another.

Much, then, remains to be done. The identity of light with a vibratory motion in electricity is henceforth something more than a seductive hypothesis; it is a probable truth. But it is not yet quite proved.

NOTE I.—Since the above was written another great step has been taken. M. Blondlot has virtually succeeded, by ingenious experimental contrivances, in directly measuring the velocity of a disturbance

along a wire. The number found differs little from the ratio of the units; that is, from the velocity of light, which is 300,000 kilometers per second. Since the interference experiments made at Geneva by Messrs. Sarasin and De la Rive have shown, as I said above, that induction is propagated in air with the same velocity as an electric disturbance which follows a conducting wire, we must conclude that the velocity of the induction is the same as that of light, which is a confirmation of the ideas of Maxwell.

M. Fizeau had formerly found for the velocity of electricity a number far smaller, about 180,000 kilometers. But there is no contradiction. The currents used by M. Fizeau, though intermittent, were of small frequency and penetrated to the axis of the wire, while the currents of M. Blondlot, oscillatory and of very short period, remained superficial and were confined to a layer of less than a hundredth of a millimeter in thickness. One may readily suppose the laws of propagation are not the same in the two cases.

NOTE II.—I have endeavored above to render the explanation of the electrostatic attractions and of the phenomena of induction comprehensible by means of a simile. Now let us see what Maxwell's idea is of the cause which produces the mutual attractions of currents.

While the electrostatic attractions are taken to be due to a multitude of little springs—that is to say, to the elasticity of the ether—it is supposed to be the living force and inertia of the same fluid which produce the phenomena of induction and electrodynamical effects.

The complete calculation is far too extended for these pages, and I shall again content myself with a simile. I shall borrow it from a well known instrument—the centrifugal governor.

The living force of this apparatus is proportional to the square of the angular velocity and to the square of the distance of the balls.

According to the hypothesis of Maxwell, the ether is in motion in galvanic currents, and its living force is proportional to the square of the intensity of the current, which thus correspond, in the parallel I am endeavoring to establish, to the angular velocity of rotation.

If we consider two currents in the same direction, the living force, with equal intensity, will be greater the nearer the currents are to one another. If the currents have opposite directions, the living force will be greater the farther they are apart.

In order to increase the angular velocity of the regulator and con-

sequently its living force, it is necessary to supply it with energy and consequently to overcome a resistance which we call its inertia.

In the same way, in order to increase the intensity of a current, we must augment the living force of the ether, and it will be necessary to supply it with energy and to overcome a resistance which is nothing but the inertia of the ether and which we call the induction.

The living force will be greater if the currents are in the same direction and near together. The energy to be furnished the counter electromotive force of induction will be greater. This is what we express when we say that the mutual action of two currents is to be added to their self-induction. The contrary is the case when their directions are opposite.

If we separate the balls of the regulator, it will be necessary, in order to maintain the angular velocity, to furnish energy, because with equal angular velocity the living force is greater the more the balls are separated.

In the same way, if two currents have the same direction and are brought toward one another, it will be necessary, in order to maintain the intensity to supply energy, because the living force will be augmented. We shall, therefore, have to overcome an electromotive force of induction which will tend to diminish the intensity of the currents. It would tend on the contrary to augment it, if the currents had the same direction and were carried apart, or if they had opposite directions and were brought together.

Finally, the centrifugal force tends to increase the distance between the balls, which would augment the living force were the angular velocity to be maintained.

In like manner, when the currents have the same direction, they attract each other—that is to say, they tend to approach each other, which would increase the living force if the intensity were maintained. If their directions are opposed they repel one another and tend to separate, which would again tend to increase the living force were the intensity kept constant.

Thus the electrostatic effects would be due to the elasticity of the ether and the electrodynamical phenomena to the living force. Now, ought this elasticity itself to be explained, as Lord Kelvin thinks, by rotations of small parts of the fluid? Different reasons may render

this hypothesis attractive; but it plays no essential part in the theory of Maxwell, which is quite independent of it.

In the same way, I have made comparisons with divers mechanisms. But they are only similes, and pretty rough ones. A complete mechanical explanation of electrical phenomena is not to be sought in the volumes of Maxwell, but only a statement of the conditions which any such explanation has to satisfy. Precisely what will confer long life on the work of Maxwell is its being unentangled with any special mechanical hypothesis.

XXXIV

AUGUST WEISMANN

1834-1914

August Weismann was born at Frankfort-on-Main, January 17, 1834, and studied medicine at Göttingen, 1852-1856. He was physician to the Austrian Archduke for two years (1860-62), but was compelled to retire because of his poor eyesight. He was called to the chair of zoology at Freiburg University. After a close study of Darwin's theory, he published in 1876 his "Studies in the Theories of Descent," a book which at once attracted much attention among scientists, for it proposed the theory of the germ-plasm as the basis of heredity, and denied the theory of the transmissibility of acquired characteristics. He died at Freiburg-in-Baden, November 6, 1914.

THE CONTINUITY OF THE GERM-PLASM AS THE FOUNDATION OF A THEORY OF HEREDITY *

INTRODUCTION

When we see that, in the higher organisms, the smallest structural details, and the most minute peculiarities of bodily and mental disposition, are transmitted from one generation to another; when we find in all species of plants and animals a thousand characteristic peculiarities of structure continued unchanged through long series of generations; when we even see them in many cases unchanged throughout whole geological periods; we very naturally ask for the causes of such a striking phenomenon: and inquire how it is that such facts become possible, how it is that the individual is able to transmit its structural fea-

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tures to its offspring with such precision. And the immediate answer to such a question must be given in the following terms:—"A single cell out of the millions of diversely differentiated cells which compose the body, becomes specialized as a sexual cell; it is thrown off from the organism and is capable of reproducing all the peculiarities of the parent body, in the new individual which springs from it by cell-division and the complex process of differentiation." Then the more precise question follows: "How is it that such a single cell can reproduce the *tout ensemble* of the parent with all the faithfulness of a portrait?"

The answer is extremely difficult; and no one of the many attempts to solve the problem can be looked upon as satisfactory; no one of them can be regarded as even the beginning of a solution or as a secure foundation from which a complete solution may be expected in the future. Neither Hæeckel's "Perigenesis of the Plastidule," nor Darwin's "Pangenesis," can be regarded as such a beginning. The former hypothesis does not really treat of that part of the problem which is here placed in the foreground, viz., the explanation of the fact that the tendencies of heredity are present in single cells, but it is rather concerned with the question as to the manner in which it is possible to conceive the transmission of a certain tendency of development into the sexual cell, and ultimately into the organism arising from it. The same may be said of the hypothesis of His, who, like Hæeckel regards heredity as the transmission of certain kinds of motion. On the other hand, it must be conceded that Darwin's hypothesis goes to the very root of the question, but he is content to give, as it were, a provisional or purely formal solution, which, as he himself says, does not claim to afford insight into the real phenomena, but only to give us the opportunity of looking at all the facts of heredity from a common standpoint. It has achieved this end, and I believe it has unconsciously done more, in that the thoroughly logical application of its principles has shown that the real causes of heredity cannot lie in the formation of gemmules or in any allied phenomena. The improbabilities to which any such theory would lead are so great that we can affirm with certainty that its details cannot accord with existing facts. Furthermore, Brooks' well-considered and brilliant attempt to modify the theory of Pangenesis cannot escape the reproach that it is based upon possibilities, which one might certainly describe as improbabilities.

But although I am of the opinion that the whole foundation of the theory of Pangenesis, however it may be modified, must be abandoned, I think, nevertheless, its author deserves great credit, and that its production has been one of those indirect roads along which science has been compelled to travel in order to arrive at the truth. Pangenesis is a modern revival of the oldest theory of heredity, that of Democritus, according to which the sperm is secreted from all parts of the body of both sexes during copulation, and is animated by a bodily force; according to this theory also, the sperm from each part of the body reproduces the same part.

If, according to the received physiological and morphological ideas of the day, it is impossible to imagine that gemmules produced by each cell of the organism are at all times to be found in all parts of the body, and furthermore that these gemmules are collected in the sexual cells, which are then able to reproduce again in a certain order each separate cell of the organism, so that each sexual cell is capable of developing into the likeness of the parent body; if all this is inconceivable, we must inquire for some other way in which we can arrive at a foundation for the true understanding of heredity. My present task is not to deal with the whole question of heredity, but only with the single although fundamental question—"How is it that a single cell of the body can contain within itself all the hereditary tendencies of the whole organism?" I am here leaving out of account the further question as to the forces and the mechanism by which these tendencies are developed in the building-up of the organism. On this account I abstain from considering at present the views of Nägeli, for as will be shown later on, they only slightly touch this fundamental question, although they may certainly claim to be of the highest importance with respect to the further question alluded to above.

Now if it is impossible for the germ-cell to be, as it were, an extract of the whole body, and for all the cells of the organism to dispatch small particles to the germ-cells, from which the latter derive their power of heredity; then there remain, as it seems to me, only two other possible, physiologically conceivable, theories as to the origin of germ-cells, manifesting such powers as we know they possess. Either the substance of the parent germ-cell is capable of undergoing a series of changes which, after the building-up of a new individual leads back again to identical germ-cells; or the germ-cells are not derived at all,

as far as their essential and characteristic substance is concerned, from the body of the individual, but they are derived directly from the parent germ-cell.

I believe that the latter view is the true one: I have expounded it for a number of years, and have attempted to defend it, and to work out its further details in various publications. I propose to call it the theory of "The Continuity of the Germ-plasm," for it is founded upon the idea that heredity is brought about by the transference from one generation to another of a substance with a definite chemical, and above all, molecular constitution. I have called this substance "germ-plasm," and have assumed that it possesses a highly complex structure, conferring upon it the power of developing into a complex organism. I have attempted to explain heredity by supposing that in each ontogeny a part of the specific germ-plasm contained in the parent egg-cell is not used up in the construction of the body of the offspring, but is reserved unchanged for the formation of the germ-cells of the following generation.

It is clear that this view of the origin of germ-cells explains the phenomena of heredity very simply, inasmuch as heredity becomes thus a question of growth and of assimilation,—the most fundamental of all vital phenomena. If the germ-cells of successive generations are directly continuous, and thus only form, as it were, different parts of the same substance, it follows that these cells must, or at any rate may, possess the same molecular constitution, and that they would therefore pass through exactly the same stages under certain conditions of development, and would form the same final product. The hypothesis of the continuity of the germ-plasm gives an identical starting point to each successive generation, and thus explains how it is that an identical product arises from all of them. In other words, the hypothesis explains heredity as part of the underlying problems of assimilation and of the causes which act directly during ontogeny; it therefore builds a foundation from which the explanation of these phenomena can be attempted.

It is true that this theory also meets with difficulties, for it seems to be unable to do justice to a certain class of phenomena, viz., the transmission of so-called acquired characters. I therefore gave immediate and special attention to this point in my first publication on heredity, and I believe that I have shown that the hypothesis of the transmis-

sion of acquired characters—up to that time generally accepted—is, to say the least, very far from being proved, and that entire classes of facts which have been interpreted under this hypothesis may be quite as well interpreted otherwise, while in many cases they must be explained differently. I have shown that there is no ascertained fact which, at least up to the present time, remains in irrevocable conflict with the hypothesis of the continuity of the germ-plasm; and I do not know any reason why I should modify this opinion to-day, for I have not heard of any objection which appears to be feasible. E. Roth has objected that in pathology we everywhere meet with the fact that acquired local disease may be transmitted to the offspring as a predisposition; but all such cases are exposed to the serious criticism that the very point that first needs to be placed on a secure footing is incapable of proof, viz., the hypothesis that the causes which in each particular case led to the predisposition were really acquired. It is not my intention, on the present occasion, to enter fully into the question of acquired characters; I hope to be able to consider the subject in greater detail at a future date. But in the meantime I should wish to point out that we ought, above all, to be clear as to what we really mean by the expression “acquired character.” An organism cannot acquire anything unless it already possesses the predisposition to acquire it: acquired characters are therefore no more than local or sometimes general variations which arise under the stimulus provided by certain external influences. If by the long-continued handling of a rifle, the so-called “*Exercierknochen*” (a bony growth caused by the pressure of the weapon in drilling) is developed, such a result depends upon the fact that the bone in question, like every other bone, contains within itself a predisposition to react upon certain mechanical stimuli, by growth in a certain direction and to a certain extent. The predisposition towards an “*Exercierknochen*” is therefore already present, or else the growth could not be formed; and the same reasoning applies to all other “acquired characters.”

Nothing can arise in an organism unless the predisposition to it is pre-existent, for every acquired character is simply the reaction of the organism upon a certain stimulus. Hence I should never have thought of asserting that predispositions cannot be transmitted, as E. Roth appears to believe. For instance, I freely admit that the predisposition to an “*Exercierknochen*” varies, and that a strongly marked

predisposition may be transmitted from father to son, in the form of bony tissue with a more susceptible constitution. But I should deny that the son could develop an "*Exercierknochen*" without having drilled, or that, after having drilled, he could develop it more easily than his father, on account of the drilling through which the latter first acquired it. I believe that this is as impossible as that the leaf of an oak should produce a gall without having been pierced by a gall-producing insect, as a result of the thousands of antecedent generations of oaks which have been pierced by such insects, and have thus "acquired" the power of producing galls. I am also far from asserting that the germ-plasm—which, as I hold, is transmitted as the basis of heredity from one generation to another—is absolutely unchangeable or totally uninfluenced by forces residing in the organism within which it is transformed into germ-cells. I am also compelled to admit that it is conceivable that organisms may exert a modifying influence upon their germ-cells, and even that such a process is to a certain extent inevitable. The nutrition and growth of the individual must exercise some influence upon its germ-cells; but in the first place this influence must be extremely slight, and in the second place it cannot act in the manner in which it is usually assumed that it takes place. A change of growth at the periphery of an organism, as in the case of an "*Exercierknochen*," can never cause such a change in the molecular structure of the germ-plasm as would augment the predisposition to an "*Exercierknochen*," so that the son would inherit an increased susceptibility of the bony tissue or even of the particular bone in question. But any change produced will result from the reaction of the germ-cell upon changes of nutrition caused by alteration in growth at the periphery, leading to some change in the size, number, or arrangement of its molecular units. In the present state of our knowledge there is reason for doubting whether such reaction can occur at all; but, if it can take place, at all events the quality of the change in the germ-plasm can have nothing to do with the quality of the acquired character, but only with the way in which the general nutrition is influenced by the latter. In the case of the "*Exercierknochen*" there would be practically no change in the general nutrition, but if such a bony growth could reach the size of a carcinoma, it is conceivable that a disturbance of the general nutrition of the body might ensue. Certain experiments on plants—on which Nägeli showed that they can be

submitted to strongly varied conditions of nutrition for several generations, without the production of any visible hereditary change—show that the influence of nutrition upon the germ-cells must be very slight, and that it may possibly leave the molecular structure of the germ-plasm altogether untouched. This conclusion is also supported by comparing the uncertainty of these results with the remarkable precision with which heredity acts in the case of those characters which are known to be transmitted. In fact, up to the present time, it has never been proved that any changes in general nutrition can modify the molecular structure of the germ-plasm, and far less has it been rendered by any means probable that the germ-cells can be affected by acquired changes which have no influence on general nutrition. If we consider that each so-called predisposition (that is, a power of reacting upon a certain stimulus in a certain way, possessed by any organism or by one of its parts) must be innate, and further that each acquired character is only the predisposed reaction of some part of an organism upon some external influence; then we must admit that only one of the causes which produce any acquired character can be transmitted, the one which was present before the character itself appeared, viz., the predisposition; and we must further admit that the latter arises from the germ, and that it is quite immaterial to the following generation whether such predisposition comes into operation or not. The continuity of the germ-plasm is amply sufficient to account for such a phenomenon, and I do not believe that any objection to my hypothesis, founded upon the actually observed phenomena of heredity, will be found to hold. If it be accepted, many facts will appear in a light different from that which has been cast upon them by the hypothesis which has been hitherto received,—a hypothesis which assumes that the organism produces germ-cells afresh, again and again, and that it produces them entirely from its own substance. Under the former theory the germ-cells are no longer looked upon as the product of the parent's body, at least as far as their essential part—the specific germ-plasm—is concerned: they are rather considered as something which is to be placed in contrast with the *tout ensemble* of the cells which make up the parent's body, and the germ-cells of succeeding generations stand in a similar relation to one another as a series of generations of unicellular organisms, arising by a continued process of cell-division. It is true that in most cases the generations of germ-

cells do not arise immediately from one another as complete cells, but only as minute particles of germ-plasm. This latter substance, however forms the foundation of the germ-cells of the next generation, and stamps them with their specific character. Previous to the publication of my theory, C. Jäger, and later M. Nussbaum, have expressed ideas upon heredity which come very near to my own. Both of these writers started with the hypothesis that there must be a direct connection between the germ-cells of succeeding generations, and they tried to establish such a continuity by supposing that the germ-cells of the offspring are separated from the parent germ-cell before the beginning of embryonic development, or at least before any histological differentiation has taken place. In this form their suggestion cannot be maintained, for it is in conflict with numerous facts. A continuity of the germ-cells does not now take place, except in very rare instances; but this fact does not prevent us from adopting a theory of the continuity of the germ-plasm, in favour of which much weighty evidence can be brought forward. In the following pages I shall attempt to develop further the theory of which I have just given a short account, to defend it against any objections which have been brought forward, and to draw from it new conclusions which may perhaps enable us more thoroughly to appreciate facts which are known, but imperfectly understood. It seems to me that this theory of continuity of the germ-plasm deserves at least to be examined in all its details, for it is the simplest theory upon the subject, and the one which is most obviously suggested by the facts of the case, and we shall not be justified in forsaking it for a more complex theory until proof that it can be no longer maintained is forthcoming. It does not presuppose anything except facts which can be observed at any moment, although they may not be understood,—such as assimilation, or the development of like organisms from like germs; while every other theory of heredity is founded on hypotheses which cannot be proved. It is nevertheless possible that continuity of the germ-plasm does not exist in the manner in which I imagine that it takes place, for no one can at present decide whether all the ascertained facts agree with and can be explained by it. Moreover, the ceaseless activity of research brings to light new facts every day, and I am far from maintaining that my theory may not be disproved by some of these. But even if it should have to be abandoned at a later period, it seems to

me that, at the present time, it is a necessary stage in the advancement of our knowledge, and one which must be brought forward and passed through, whether it prove right or wrong, in the future. In this spirit I offer the following considerations, and it is in this spirit that I should wish them to be received.

THE GERM-PLASM

I entirely agree with Strasburger when he says, "The specific qualities of organisms are based upon nuclei"; and I further agree with him in many of his ideas as to the relation between the nucleus and cell-body: "Molecular stimuli proceed from the nucleus into the surrounding cytoplasm; stimuli which, on the one hand, control the phenomena of assimilation in the cell, and, on the other hand, give to the growth of the cytoplasm, which depends upon nutrition, a certain character peculiar to the species." "The nutritive cytoplasm assimilates, while the nucleus controls the assimilation, and hence the substances assimilated possess a certain constitution and nourish in a certain manner the cyto-idioplasm and the nuclear idioplasm. In this way the cytoplasm takes part in the phenomena of construction, upon which the specific form of the organism depends. This constructive activity of the cyto-idioplasm depends upon the regulative influence of the nuclei." The nuclei therefore "determine the specific direction in which an organism develops."

The opinion—derived from the recent study of the phenomena of fertilization—that the nucleus impresses its specific character upon the cell, has received conclusive and important confirmation in the experiments upon the regeneration of Infusoria, conducted simultaneously by M. Nussbaum at Bonn, and by A. Gruber at Freiburg. Nussbaum's statement that an artificially separated portion of a *Paramaecium*, which does not contain any nuclear substance, immediately dies, must not be accepted as of general application, for Gruber has kept similar fragments of other Infusoria alive for several days. Moreover, Gruber had previously shown that individual Protozoa occur, which live in a normal manner, and are yet without a nucleus, although this structure is present in other individuals of the same species. But the meaning of the nucleus is made clear by the fact, published by Gruber, that such artificially separated fragments of

Infusoria are incapable of regeneration, while on the other hand those fragments which contain nuclei always regenerate. It is therefore only under the influence of the nucleus that the cell substance re-develops into the full type of the species. In adopting the view that the nucleus is the factor which determines the specific nature of the cell, we stand on a firm foundation upon which we can build with security.

If therefore the first segmentation nucleus contains, in its molecular structure, the whole of the inherited tendencies of development, it must follow that during segmentation and subsequent cell-division, the nucleoplasm will enter upon definite and varied changes which must cause the differences appearing in the cells which are produced; for identical cell-bodies depend, *ceteris paribus*, upon identical nucleoplasm, and conversely different cells depend upon differences in the nucleoplasm. The fact that the embryo grows more strongly in one direction than in another, that its cell-layers are of different nature and are ultimately differentiated into various organs and tissues,—forces us to accept the conclusion that the nuclear substance has also been changed in nature, and that such changes take place during ontogenetic development in a regular and definite manner. This view is also held by Strasburger, and it must be the opinion of all who seek to derive the development of inherited tendencies from the molecular structure of the germ-plasm, instead of from preformed gemmules.

We are thus led to the important question as to the forces by which the determining substance or nucleoplasm is changed, and as to the manner in which it changes during the course of ontogeny, and on the answer to this question our further conclusions must depend. The simplest hypothesis would be to suppose that, at each division of the nucleus, its specific substance divides into two halves of unequal quality, so that the cell-bodies would also be transformed; for we have seen that the character of a cell is determined by that of its nucleus. Thus in any Metazoon the first two segmentation spheres would be transformed in such a manner that one only contained the hereditary tendencies of the endoderm and the other those of the ectoderm, and therefore, at a later stage, the cells of the endoderm would arise from the one and those of the ectoderm from the other; and this is actually known to occur. In the course of further division the nucleoplasm of the first ectoderm cell would again divide unequally, *e. g.*, into the

nucleoplasm containing the hereditary tendencies of the nervous system, and into that containing the tendencies of the external skin. But even then, the end of the unequal division of nuclei would not have been nearly reached; for, in the formation of the nervous system, the nuclear substance which contains the hereditary tendencies of the sense-organs would, in the course of further cell-division, be separated from that which contains the tendencies of the central organs, and the same process would continue in the formation of all single organs, and in the final development of the most minute histological elements. This process would take place in a definitely ordered course, exactly as it has taken place throughout a very long series of ancestors; and the determining and directing factor is simply and solely the nuclear substance, the nucleoplasm, which possesses such a molecular structure in the germ-cell that all such succeeding stages of its molecular structure in future nuclei must necessarily arise from it, as soon as the requisite external conditions are present. This is almost the same conception of ontogenetic development as that which has been held by embryologists who have not accepted the doctrine of evolution: for we have only to transfer the primary cause of development, from an unknown source within the organism, into the nuclear substance, in order to make the views identical.

I believe I have shown that theoretically hardly any objection can be raised against the view that the nuclear substance of somatic cells may contain unchanged germ-plasm, or that this germ-plasm may be transmitted along certain lines. It is true that we might imagine *a priori* that all somatic nuclei contain a small amount of unchanged germ-plasm. In Hydroids such an assumption cannot be made, because only certain cells in a certain succession possess the power of developing into germ-cells; but it might well be imagined that in some organisms it would be a great advantage if every part possessed the power of growing up into the whole organism and of producing sexual cells under appropriate circumstances. Such cases might exist if it were possible for all somatic nuclei to contain a minute fraction of unchanged germ-plasm. For this reason, Strasburger's other objection against my theory also fails to hold; viz., that certain plants can be propagated by pieces of rhizomes, roots, or even by means of leaves, and that plants produced in this manner may finally give rise

to flowers, fruit and seeds, from which new plants arise. "It is easy to grow new plants from the leaves of begonia which have been cut off and merely laid upon moist sand, and yet in the normal course of ontogeny the molecules of germ-plasm would not have been compelled to pass through the leaf; and they ought therefore to be absent from its tissue. Since it is possible to raise from the leaf a plant which produces flower and fruit, it is perfectly certain that special cells containing the germ-substance cannot exist in the plant." But I think that this fact only proves that in begonia and similar plants all the cells of the leaves or perhaps only certain cells contain a small amount of germ-plasm, and that consequently these plants are specially adapted for propagation by leaves. How is it then that all plants cannot be reproduced in this way? No one has ever grown a tree from the leaf of the lime or oak, or a flowering plant from the leaf of the tulip or convolvulus. It is insufficient to reply that in the last mentioned cases the leaves are more strongly specialized, and have thus become unable to produce germ-substance; for the leaf-cells in these different plants have hardly undergone histological differentiation in different degrees. If, notwithstanding, the one can produce a flowering plant, while the others have not this power, it is of course clear that reasons other than the degree of histological differentiation must exist; and, according to my opinion, such a reason is to be found in the admixture of a minute quantity of unchanged germ-plasm with some of their nuclei.

In Sach's excellent lectures on the physiology of plants, we read on page 723—"In the true mosses almost any cell of the roots, leaves and shoot-axes, and even of the immature sporogonium, may grow out under favourable conditions, become rooted, form new shoots, and give rise to an independent living plant." Since such plants produce germ-cells at a later period, we have here a case which requires the assumption that all or nearly all cells must contain germ-plasm.

The theory of the continuity of the germ-plasm seems to me to be still less disproved or even rendered improbable by the facts of the alternation of generations. If the germ-plasm may pass on from the egg into certain somatic cells of an individual, and if it can be further transmitted along certain lines, there is no difficulty in supposing that it may be transmitted through a second, third, or through any number of individuals produced from the former by budding. In fact, in

the Hydroids, on which my theory of the continuity of the germ-plasm has been chiefly based, alternation of generations is the most important means of propagation.

THE SIGNIFICANCE OF THE POLAR BODIES

We have already seen that the specific nature of a cell depends upon the molecular structure of its nucleus; and it follows from this conclusion that my theory is further, and as I believe strongly, supported, by the phenomenon of the expulsion of polar bodies, which has remained inexplicable for so long a time.

For if the specific molecular structure of a cell-body is caused and determined by the structure of the nucleoplasm, every kind of cell which is histologically differentiated must have a specific nucleoplasm. But the egg-cell of most animals, at any rate during the period of growth, is by no means an indifferent cell of the most primitive type. At such a period its cell-body has to perform quite peculiar and specific functions; it has to secrete nutritive substances of a certain chemical nature and physical constitution, and to store up this food material in such a manner that it may be at the disposal of the embryo during its development. In most cases the egg-cell also forms membranes which are often characteristic of particular species of animals. The growing egg-cell is therefore histologically differentiated: and in this respect resembles a somatic cell. It may perhaps be compared to a gland-cell, which does not expel its secretion, but deposits it within its own substance. To perform such specific functions it requires a specific cell-body, and the latter depends upon a specific nucleus. It therefore follows that the growing egg-cell must possess nucleoplasm of specific molecular structure, which directs the above mentioned secretory functions of the cell. The nucleoplasm of histologically differentiated cells may be called histogenetic nucleoplasm, and the growing egg-cell must contain such a substance, and even a certain specific modification of it. This nucleoplasm cannot possibly be the same as that which, at a later period, causes embryonic development. Such development can only be produced by the true germ-plasm of immensely complex constitution, such as I have previously attempted to describe. It therefore follows that the nucleus of the egg-cell contains two kinds of nucleoplasm:—germ and a peculiar modi-

fication of histogenetic nucleoplasm, which may be called ovogenetic nucleoplasm. This substance must greatly preponderate in the young egg-cell, for, as we have already seen, it controls the growth of the latter. The germ-plasm, on the other hand, can only be present in minute quantity at first, but it must undergo considerable increase during the growth of the cell. But in order that the germ-plasm may control the cell-body, or, in other words, in order that embryonic development may begin, the still preponderating ovogenetic nucleoplasm must be removed from the cell. This removal takes place in the same manner as that in which differing nuclear substances are separated during the ontogeny of the embryo: viz., by nuclear division, leading to cell-division. The expulsion of the polar bodies is nothing more than the removal of ovogenetic nucleoplasm from the egg-cell. That the ovogenetic nucleoplasm continues greatly to preponderate in the nucleus up to the very last, may be concluded from the fact that two successive divisions of the latter and the expulsion of two polar bodies appear to be the rule. If in this way a small part of the cell-body is expelled from the egg, the extrusion must in all probability be considered as an inevitable loss, without which the removal of the ovogenetic nucleoplasm cannot be effected.

ON THE NATURE OF PARTHOGENESIS

It is well known that the formation of polar bodies has been repeatedly connected with the sexuality of germ-cells, and that it has been employed to explain the phenomena of parthenogenesis. I may now perhaps be allowed to develop the views as to the nature of parthenogenesis at which I have arrived under the influence of my explanation of polar bodies.

The theory of parthenogenesis adopted by Minot and Balfour is distinguished by its simplicity and clearness, among all other interpretations which had been hitherto offered. Indeed, their explanation follows naturally and almost as a matter of course, if the assumption made by these observers be correct, that the polar body is the male part of the hermaphrodite egg-cell. An egg which has lost its male part cannot develop into an embryo until it has received a new male part in fertilization. On the other hand, an egg which does not expel its male part may develop without fertilization, and thus we are led

to the obvious conclusion that parthenogenesis is based upon the non-expulsion of polar bodies. Balfour distinctly states "that the function of forming polar cells has been acquired by the ovum for the express purpose of preventing parthenogenesis."

It is obvious that I cannot share this opinion, for I regard the expulsion of polar bodies as merely the removal of the ovogenetic nucleoplasm, on which depended the development of the specific histological structure of the egg-cell. I must assume that the phenomena of maturation in the parthenogenetic egg and in the sexual egg are precisely identical, and that in both, the ovogenetic nucleoplasm must in some way be removed before embryonic development can begin.

Unfortunately the actual proof of this assumption is not so complete as might be desired. In the first place, we are as yet uncertain whether polar bodies are or are not expelled by parthenogenetic eggs; for in no single instance has such expulsion been established beyond doubt. It is true that this deficiency does not afford any support to the explanation of Minot and Balfour, for in all cases in which polar bodies have not been found in parthenogenetic eggs, these structures are also absent from the eggs which require fertilization in the same species. But although the expulsion of polar bodies in parthenogenesis has not yet been proved to occur, we must assume it to be nearly certain that the phenomena of maturation, whether connected or unconnected with the expulsion of polar bodies, are the same in the eggs which develop parthenogenetically and in those which are capable of fertilization, in one and the same species. This conclusion depends, above all, upon the phenomena of reproduction in bees, in which, as a matter of fact, the same egg may be fertilized or may develop parthenogenetically, as I shall have occasion to describe in greater detail at a later period.

Hence when we see that the eggs of many animals are capable of developing without fertilization, while in other animals such development is impossible, the difference between the two kinds of eggs must rest upon something more than the mode of transformation of the nucleus of the germ-cell into the first segmentation nucleus. There are, indeed, facts which distinctly point to the conclusion that the difference is based upon quantitative and not qualitative relations. A large number of insects are exceptionally reproduced by the parthenogenetic method, *e. g.*, in Lepidoptera. Such development does

not take place in all the eggs laid by an unfertilized female, but only in part, and generally a small fraction of the whole, while the rest die. But among the latter there are some which enter upon embryonic development without being able to complete it, and the stage at which development may cease also varies. It is also known that the eggs of higher animals may pass through the first stages of segmentation without having been fertilized. This was shown to be the case in the egg of the frog by Leuckart, in that of the fowl by Oellacher, and even in the egg of mammals by Hensen.

Hence in such cases it is not the impulse to development, but the power to complete it, which is absent. We know that force is always bound up with matter, and it seems to me that such instances are best explained by the supposition that too small an amount of that form of matter is present, which, by its controlling agency, effects the building up of the embryo by the transformation of mere nutritive material. This substance is the germ-plasm of the segmentation nucleus, and I have assumed above that it is altered in the course of ontogeny by changes which arise from within, so that when sufficient nourishment is afforded by the cell-body, each succeeding stage necessarily results from the preceding one. I believe that changes arise in the constitution of the nucleoplasm at each cell-division which takes place during the building up of the embryo, changes which either correspond or differ in the two halves of each nucleus. If, for the present, we neglect the minute amount of unchanged germ-plasm which is reserved for the formation of the germ-cells, it is clear that a great many different stages in the development of somatic nucleoplasm are thus formed, which may be denominated as stages 1, 2, 3, 4, etc., up to n . In each of these stages the cells differ more as development proceeds, and as the number by which the stage is denominated becomes higher. Thus, for instance, the two first segmentation spheres would represent the first stage of somatic nucleoplasm, a stage which may be considered as but slightly different in its molecular structure from the nucleoplasm of the segmentation nucleus; the first four segmentation spheres would represent the second stage; the succeeding eight spheres the third, and so on. It is clear that at each successive stage the molecular structure of the nucleoplasm must be further removed from that of the germ-plasm, and that, at the same time, the cells of each successive stage must also diverge more widely

among themselves in the molecular structure of their nucleoplasm. Early in development each cell must possess its own peculiar nucleoplasm, for the further course of development is peculiar to each cell. It is only in the later stages that equivalent or nearly equivalent cells are formed in large numbers, cells in which we must also suppose the existence of equivalent nucleoplasm.

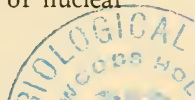
If we may assume that a certain amount of germ-plasm must be contained in the segmentation nucleus in order to complete the whole process of the ontogenetic differentiation of this substance; if we may further assume that the quantity of germ-plasm in the segmentation nucleus varies in different cases; then we should be able to understand why one egg can only develop after fertilization, while another can begin its development without fertilization, but cannot finish it, and why a third is even able to complete its development. We should also understand why one egg only passes through the first stages of segmentation and is then arrested, while another reaches a few more stages in advance, and a third develops so far that the embryo is nearly completely formed. These differences would depend upon the extent to which the germ-plasm, originally present in the egg, was sufficient for the development of the latter; development will be arrested as soon as the nucleoplasm is no longer capable of producing the succeeding stage, and is thus unable to enter upon the following nuclear division.

From a general point of view such a theory would explain many difficulties, and it would render possible an explanation of the phyletic origin of parthenogenesis, and an adequate understanding of the strange and often apparently abrupt and arbitrary manner of its occurrence. In my works on *Daphnidae* I have already laid especial stress upon the proposition that parthenogenesis in insects and Crustacea certainly cannot be an ancestral condition which has been transmitted by heredity, but that it has been derived from a sexual condition. In what other way can we explain the fact that parthenogenesis is present in certain species or genera, but absent in others closely allied to them; or the fact that males are entirely wanting in species of which the females possess a complete apparatus for fertilization? I will not repeat all the arguments with which I attempted to support this conclusion. Such a conclusion may be almost certainly accepted for the *Daphnidae*, because parthenogenesis does not occur

in their still living ancestors, the Phyllopods, and especially the Estheridae. In Daphnidae the cause and object of the phyletic development of parthenogenesis may be traced more clearly than in any other group of animals. In Daphnidae we can accept the conclusion with greater certainty than in all other groups, except perhaps the Aphidae, that parthenogenesis is extremely advantageous to species in certain conditions of life; and that it has only been adopted when, and as far as, it has been beneficial; and further, that at least in this group parthenogenesis became possible and was adopted in each species as soon as it became useful. Such a result can be easily understood if it is only the presence of more or less germ-plasm which decides whether an egg is or is not capable of development without fertilization.

If we now examine the foundations of this hypothesis we shall find that we may at once accept one of its assumptions, viz., that fluctuations occur in the quantity of germ-plasm in the segmentation nucleus; for there can never be absolute equality in any single part of different individuals. As soon therefore as these fluctuations become so great that parthenogenesis is produced, it may become, by the operation of natural selection, the chief mode of reproduction of the species or of certain generations of the species. In order to place this theory upon a firm basis, we have simply to decide whether the quantity of germ-plasm contained in the segmentation nucleus is the factor which determines development; although for the present it will be sufficient if we can render this view to some extent probable, and show that it is not a contradiction of established facts.

At first sight this hypothesis seems to encounter serious difficulties. It will be objected that neither the beginning nor the end of embryonic development can possibly depend upon the quantity of nucleoplasm in the segmentation nucleus, since the amount may be continually increased by growth; for it is well known that during embryonic development the nuclear substance increases with astonishing rapidity. By an approximate calculation I found that in the egg of a *Cynips* the quantity of nuclear substance present at the time when the blastoderm was about to be formed, and when there were twenty-six nuclei, was even then seven times as great as the quantity which had been contained in the segmentation nucleus. How then can we imagine that embryonic development would ever be arrested from want of nuclear



substance, and if such deficiency really acted as an arresting force, how then could development begin at all? We might suppose that when germ-plasm is present in sufficient quantity to start segmentation, it must also be sufficient to complete the development; for it grows continuously, and must presumably always possess a power equal to that which it possessed at the beginning, and which was just sufficient to start the process of segmentation. If at each ontogenetic stage the quantity of nucleoplasm is just sufficient to produce the following stage, we might well imagine that the whole ontogeny would necessarily be completed.

The flaw in this argument lies in the erroneous assumption that the growth of nuclear substance is, when the quality of the nucleus and the conditions of nutrition are equal, unlimited and uncontrolled. The intensity of growth must depend upon the quantity of nuclear substance with which growth and the phenomena of segmentation commenced. There must be an optimum quantity of nucleoplasm with which the growth of the nucleus proceeds most favourably and rapidly, and this optimum will be represented in the normal size of the segmentation nucleus. Such a size is just sufficient to produce, in a certain time and under certain external conditions, the nuclear substance necessary for the construction of the embryo, and to start the long series of cell-divisions. When the segmentation nucleus is smaller, but large enough to enter upon segmentation, the nuclei of the two first embryonic cells will fall rather more below the normal size, because the growth of the segmentation nucleus, during and after division will be less rapid on account of its unusually small size. The succeeding generations of nuclei will depart more and more from the normal size in each respective stage, because they do not pass into a resting stage during embryonic development, but divide again immediately after their formation. Hence nuclear growth would become less vigorous as the nuclei fell more and more below the optimum size, and at last a moment would arrive when they would be unable to divide, or would be at least unable to control the cell-body in such a manner as to lead to its division.

The first event of importance for embryonic development is the maturation of the egg, *i. e.*, the transformation of the nucleus of the germ-cell into a nuclear spindle and the removal of the ovogenetic nucleoplasm by the separation of polar bodies, or by some analogous

process. There must be some cause for this separation, and I have already tried to show that it may lie in the quantitative relations which obtain between the two kinds of nucleoplasm contained in the nucleus of the egg. I have suggested that the germ-plasm, at first small in quantity, undergoes a gradual increase, so that it can finally oppose the ovogenetic nucleoplasm. I will not further elaborate this suggestion, for the ascertained facts are insufficient for the purpose. But the appearances witnessed in nuclear division indicate that there are opposing forces, and that such a contest is the motive cause of division; and Roux may be right in referring the opposition to electrical forces. However this may be, it is perfectly certain that the development of this opposition is based upon internal conditions arising during growth in the nucleus itself. The quantity of nuclear thread cannot by itself determine whether the nucleus can or cannot enter upon division; if so, it would be impossible for two divisions to follow each other in rapid succession, as is actually the case in the separation of the two polar bodies, and also in their subsequent division. In addition to the effects of quantity, the internal conditions of the nucleus must also play an important part in these phenomena. Quantity alone does not necessarily produce nuclear division, or the nucleus of the egg would divide long before maturation is complete, for it contains much more nucleoplasm than the female pronucleus, which remains in the egg after the expulsion of the polar bodies, and which is in most cases capable of further division. But the fact that segmentation begins immediately after the conjugation of male and female pronuclei, also shows that quantity is an essential requisite. The effect of fertilization has been represented as analogous to that of the spark which kindles the gunpowder. In the latter case an explosion ensues, in the former segmentation begins. Even now many authorities are inclined to refer the polar repulsion manifested in the nuclear division which immediately follows fertilization, to the antagonism between male and female elements. But, according to the important discoveries of Flemming and van Beneden, the polar repulsion in each nuclear division is not based on the antagonism between male and female loops, but depends upon the antagonism and mutual repulsion between the two halves of the same loop. The loops of the father and those of the mother remain together and divide together throughout the whole ontogeny.

What can be the explanation of the fact that nuclear division follows immediately after fertilization, but that without fertilization it does not occur in most cases? There is only one possible explanation, viz., the fact that the quantity of the nucleus has been suddenly doubled, as the result of conjugation. The difference between the male and female pronuclei cannot serve as an explanation, even though the nature of this difference is entirely unknown, because polar repulsion is not developed between the male and female halves of the nucleus, but within each male and each female half. We are thus forced to conclude that increase in the quantity of the nucleus affords an impulse for division, the disposition towards it being already present. It seems to me that this view does not encounter any theoretical difficulties, and that it is an entirely feasible hypothesis to suppose that, besides the internal conditions of the nucleus, its quantitative relation to the cell-body must be taken into especial account. It is imaginable, or perhaps even probable, that the nucleus enters upon division as soon as its idioplasm has attained a certain strength, quite apart from the supposition that certain internal conditions are necessary for this end. As above stated, such conditions may be present, but division may not occur because the right quantitative relation between nucleus and cell-body, or between the different kinds of nuclear idioplasm has not been established. I imagine that such a quantitative deficiency exists in an egg which, after the expulsion of the ovogenetic nucleoplasm in the polar bodies, requires fertilization in order to begin segmentation. The fact that the polar bodies were expelled proves that the quantity of the nucleus was sufficient to cause division, while afterwards it was no longer sufficient to produce such a result.

This suggestion will be made still clearer by an example. In *Ascaris megalocephala* the nuclear substance of the female pronucleus forms two loops, and the male pronucleus does the same; hence the segmentation nucleus contains four loops, and this is also the case with the first segmentation spheres. If we suppose that in embryonic development the first nuclear division requires such an amount of nuclear substance as is necessary for the formation of four loops,—it follows that an egg, which can only form two or three loops from its nuclear reticulum, would not be able to develop parthenogenetically, and that not even the first division would take place. If we further

suppose that, while four loops are sufficient to start nuclear division, these loops must be of a certain size and quantity in order to complete the whole ontogeny (in a certain species), it follows that eggs possessing a reticulum which contains barely enough nuclear substance to divide into four segments, would be able to produce the first division and perhaps also the second and third, or some later division, but that at a certain point during ontogeny, the nuclear substance would become insufficient, and development would be arrested. This will occur in eggs which enter upon development without fertilization, but are arrested before its completion. One might compare this retardation leading to the final arrest of development, to a railway train which is intended to meet a number of other trains at various junctions, and which can only travel slowly because of some defect in the engine. It will be a little behind time at the first junction, but it may just catch the train, and it may also catch the second or even the third; but it will be later at each successive junction, and will finally arrive too late for a certain train; and after that it will miss all the trains at the remaining junctions. The nuclear substance grows continuously during development, but the rate at which it increases depends upon the nutritive conditions together with its initial quantity. The nutritive changes during the development of an egg depend upon the quantity of the cell-body which was present at the outset, and which cannot be increased. If the quantity of the nuclear substance is rather too small at the beginning, it will become more and more insufficient in succeeding stages, as its growth becomes less vigorous, and differs more from the standard it would have reached if the original quantity had been normal. Consequently it will gradually fall more and more short of the normal quantity, like the train which arrives later and later at each successive junction, because its engine, although with the full pressure of steam, is unable to attain the normal speed.

It will be objected that four loops cannot be necessary for nuclear division in *Ascaris*, since such division takes place in the formation of the polar bodies, resulting in the appearance of the female pronucleus with only two loops. But this fact only shows that the quantity of nuclear substance necessary for the formation of four loops is not necessary for all nuclear divisions; it does not disprove the assumption that such a quantity is required for the division of the

segmentation nucleus. In addition to these considerations we must not leave the substance of the cell-body altogether out of account, for, although it is not the bearer of the tendencies of heredity, it must be necessary for every change undergone by the nucleus, and it surely also possesses the power of influencing changes to a large extent. There must be some reason for the fact that in all animal eggs with which we are acquainted, the nucleus moves to the surface of the egg at the time of maturation, and there passes through its well known transformation. It is obvious that it is there subjected to different influences from those which would have acted upon it in the center of the cell-body, and it is clear that such an unequal cell-division as takes place in the separation of the polar bodies could not occur if the nucleus remained in the center of the egg.

This explanation of the necessity for fertilization does not exclude the possibility that, under certain circumstances, the substance of the egg-nucleus may be larger, so that it is capable of forming four loops. Eggs which thus possess sufficient nucleoplasm, viz., germ-plasm, for the formation of the requisite four loops of normal size (namely, of the size which would have been produced by fertilization), can and must develop by the parthenogenetic method.

Of course the assumption that four loops must be formed has only been made for the sake of illustration. We do not yet know whether there are always exactly four loops in the segmentation nucleus. I may add that, although the details by which these considerations are illustrated are based on arbitrary assumptions, the fundamental view that the development of the egg depends, *ceteris paribus*, upon the quantity of nuclear substance, is certainly right, and follows as a necessary conclusion from the ascertained facts. It is not unlikely that such a view may receive direct proof in the results of future investigations. Such proof might, for instance, be forthcoming if we were to ascertain, in the same species, the number of loops present in the segmentation nucleus of fertilization, as compared with those present in the segmentation nucleus of parthenogenesis.

The reproductive process in bees will perhaps be used as an argument against my theory. In these insects the same egg will develop into a female or male individual, according as fertilization has or has not taken place, respectively. Hence one and the same egg is capable

of fertilization, and also of parthenogenetic development, if it does not receive a spermatozoon. It is in the power of the queen-bee to produce male or female individuals: by an act of will she decides whether the egg she is laying is to be fertilized or unfertilized. She "knows beforehand" whether an egg will develop into a male or a female animal, and deposits the latter kind in the cells of queens and workers, the former in the cells of drones. It has been shown by the discoveries of Leuckart and von Siebold that all the eggs are capable of developing into male individuals, and that they are only transformed into "female eggs" by fertilization. This fact seems to be incompatible with my theory as to the cause of parthenogenesis, for if the same egg, possessing exactly the same contents, and above all the same segmentation nucleus, may develop sexually or parthenogenetically, it appears that the power of parthenogenetic development must depend on some factor other than the quantity of germ-plasm.

Although this appears to be the case, I believe that my theory encounters no real difficulty. I have no doubt whatever that the same egg may develop with or without fertilization. From a careful study of the numerous excellent investigations upon this point which have been conducted in a particularly striking manner by Bessels (in addition to the observers quoted above), I have come to the conclusion that the fact is absolutely certain. It must be candidly admitted that the same egg will develop into a drone when not fertilized, or into a worker or queen when fertilized. One of Bessels' experiments is sufficient to prove this assertion. He cut off the wings of a young queen and thus rendered her incapable of taking "the nuptial flight." He then observed that all the eggs which she laid developed into male individuals. This experiment was made in order to prove that drones are produced by unfertilized eggs; but it also proves that the assertion mentioned above is correct, for the eggs which ripen first and are therefore first laid, would have been fertilized had the queen been impregnated. The supposition that, at certain times, the queen produces eggs requiring fertilization, while at other times her eggs develop parthenogenetically, is quite excluded by this experiment; for it follows from it that the eggs must all be of precisely the same kind, and that there is no difference between the eggs which require fertilization and those which do not.

But does it therefore follow that the quantity of germ-plasm in the segmentation nucleus is not the factor which determines the beginning of embryonic development? I believe not. It can be very well imagined that the nucleus of the egg, having expelled the ovogenetic nucleoplasm, may be increased to the size requisite for the segmentation nucleus in one of two ways: either by conjugation with a sperm-nucleus, or by simply growing to double its size. There is nothing improbable in this latter assumption, and one is even inclined to inquire why such growth does not take place in all unfertilized eggs. The true answer to this question must be that nature pursues the sexual method of reproduction, and that the only way in which the general occurrence of parthenogenesis could be prevented was by the production of eggs which remained sterile unless they were fertilized. This was effected by a loss of the capability of growth on the part of the egg-nucleus after it had expelled the ovogenetic nucleoplasm.

The case of the bee proves in a very striking manner that the difference between eggs which require fertilization, and those which do not, is not produced until after the maturation of the egg and the removal of the ovogenetic nucleoplasm. The increase in the quantity of the germ-plasm cannot have taken place at any earlier period, or else the nucleus of the egg would always start embryonic development by itself, and the egg would probably be incapable of fertilization. For the relation between egg-nucleus and sperm-nucleus is obviously based upon the fact that each of them is insufficient by itself, and requires completion. If such completion had taken place at an early stage the egg-nucleus would either cease to exercise any attractive force upon the sperm-nucleus, or else conjugation would be effected, as in Fol's interesting experiments upon fertilization by many spermatozoa; and, as in these experiments, malformation of the embryo would result. In *Daphnidae* I believe I have shown that the summer eggs are not only developed parthenogenetically, but also that they are never fertilized; and the explanation of this incapacity for fertilization may perhaps be found in the fact that their segmentation nucleus is already formed.

We may therefore conclude that, in bees, the nucleus of the egg, formed during maturation, may either conjugate with the sperm-nucleus, or else if no spermatozoon reaches it the egg may, under the

stimulus of internal causes, grow to double its size, thus attaining the dimensions of the segmentation nucleus. For our present purpose we may leave out of consideration the fact that in the latter case the individual produced is a male, and in the former case a female.

XXXV

SIR NORMAN LOCKYER

1836-1920

Sir Joseph Norman Lockyer, born at Rugby, England, May 17, 1836, entered the War Office in 1857. Through his own exertions he educated himself in science and was one of the first to suggest the hypothesis that the earth and other spheres were the result of the aggregation of meteorites. He was also the first to apply the spectro-scope to the corona of the sun, revealing the chemical composition of solar prominences as chiefly hydrogen, calcium, and helium. He died at Sidmouth, Devonshire, August 16, 1920.

THE CHEMISTRY OF THE STARS *

The importance of practical work, the educational value of the seeking after truth by experiment and observation on the part of even young students, are now generally recognized. That battle has been fought and won. But there is a tendency in the official direction of seats of learning to consider what is known to be useful, because it is used, in the first place. The fact that the unknown, that is, the unstudied, is the mine from which all scientific knowledge with its million applications has been won is too often forgotten.

Bacon, who was the first to point out the importance of experiment in the physical sciences, and who predicted the applications to which I have referred, warns us that "*lucifera experimenta non fructifera quaerenda*"; and surely we should highly prize those results which enlarge the domain of human thought and help us to understand the mechanism of the wonderful universe in which our lot is cast, as well as those which add to the comfort and the convenience of our lives.

It would be also easy to show by many instances how researches,

* From an address delivered at the University of Birmingham (1900).

considered ideally useless at the time they were made, have been the origin of the most tremendous applications. One instance suffices. Faraday's trifling with wires and magnets has already landed us in one of the greatest revolutions which civilization has witnessed; and where the triumphs of electrical science will stop no man can say.

This is a case in which the useless has been rapidly sublimed into utility so far as our material wants are concerned.

I propose to bring to your notice another "useless" observation suggesting a line of inquiry which I believe sooner or later is destined profoundly to influence human thought along many lines.

Fraunhofer at the beginning of this century examined sunlight and starlight through a prism. He found that the light received from the sun differed from that of the stars. So useless did his work appear that we had to wait for half a century till any considerable advance was made. It was found at last that the strange "lines" seen and named by Fraunhofer were precious indications of the chemical substances present in worlds immeasurably remote. We had, after half a century's neglect, the foundation of solar and stellar chemistry, an advance in knowledge equaling any other in its importance.

In dealing with my subject I shall first refer to the work which has been done in more recent years with regard to this chemical conditioning of the atmospheres of stars, and afterwards very briefly show how this work carries us into still other new and wider fields of thought.

The first important matter which lies on the surface of such a general inquiry as this is that if we deal with the chemical elements as judged by the lines in their spectra we know for certain of the existence of oxygen, of nitrogen, of argon, representing one class of gases, in no celestial body whatever; whereas, representing other gases, we have a tremendous demonstration of the existence of all the known lines of hydrogen and helium.

We see, then, that the celestial sorting out of gases is quite different from the terrestrial one.

Taking the substances classed by the chemist as non-metals, we find carbon and silicium—I prefer, on account of its stellar behavior, to call it silicium, though it is old fashioned—present in celestial phenomena. We have evidence of this in the fact that we have a considerable development of carbon in some stars and an indication of silicium in others. But these are the only non-metals observed.

Now, with regard to the metallic substances which we find, we deal chiefly with calcium, strontium, iron, and magnesium. Others are not absolutely absent, but their percentage quantity is so small that they are negligible in a general statement.

Now do these chemical elements exist indiscriminately in all the celestial bodies, so that practically, from a chemical point of view, the bodies appear to us of similar chemical constitution? No; it is not so.

From the spectra of those stars which resemble the sun, in that they consist of an interior nucleus surrounded by an atmosphere which absorbs the light of the nucleus, and which, therefore, we study by means of this absorption, it is to be gathered that the atmospheres of some stars are chiefly gaseous—i. e., consisting of elements we recognize as gases here—of others chiefly metallic, of others again mainly composed of carbon or compounds of carbon.

Here, then, we have spectroscopically revealed the fact that there is considerable variation in the chemical constituents which build up the stellar atmospheres.

This, though a general, is still an isolated statement. Can we connect it with another? One of the laws formulated by Kirchhoff in the infancy of spectroscopic inquiry has to do with the kind of radiation given out by bodies at different temperatures. A poker placed in a fire first becomes red, and, as it gets hotter, white hot. Examined in a spectroscope, we find that the red condition comes from the absence of blue light; that the white condition comes from the gradual addition of blue as the temperature increases.

The law affirms that the hotter a mass of matter is the farther its spectrum extends into the ultraviolet.

Hence the hotter a star is the farther does its complete or continuous spectrum lengthen out toward the ultraviolet and the less it is absorbed by cooler vapors in its atmosphere.

Now, to deal with three of the main groups of stars, we find the following very general result:

Gaseous stars	Longest spectrum.
Metallic stars	Medium spectrum.
Carbon stars	Shortest spectrum.

We have now associated two different series of phenomena, and we are enabled to make the following statement:

Gaseous stars	Highest temperature.
Metallic stars	Medium temperature.
Carbon stars	Lowest temperature.

Hence the differences in apparent chemical constitutions are associated with differences of temperature.

Can we associate with the two to which I have already called attention still a third class of facts? Laboratory work enables us to do this. When I began my inquiries the idea was, one gas or vapor, one spectrum. We now know that this is not true; the systems of bright lines given out by radiating substances change with the temperature.

We can get the spectrum of a well known compound substance—say carbonic oxide; it is one special to the compound; we increase the temperature so as to break up the compound, and we then get the spectra of its constituents, carbon and oxygen.

But the important thing in the present connection is that the spectra of the chemical elements behave exactly in the same way as the spectra of known compounds do when we employ temperatures far higher than those which break up the compounds; and indeed in some cases the changes are more marked. For brevity I will take for purposes of illustration three substances, and deal with one increase of temperature only, a considerable one and obtainable by rendering a substance incandescent, first by a direct current of electricity, as happens in the so-called “arc lamps” employed in electric lighting, and next by the employment of a powerful induction coil and battery of Leyden jars. In laboratory parlance we pass thus from the arc to the jar-spark. In the case of magnesium, iron, and calcium, the changes observed on passing from the temperature of the arc to that of the spark have been minutely observed. In each, new lines are added or old ones are intensified at the higher temperature. Such lines have been termed “enhanced lines.”

These enhanced lines are not seen alone; outside the region of high temperature in which they are produced, the cooling vapors give us the cool lines. Still we can conceive the enhanced lines to be seen alone at the highest temperature in a space sufficiently shielded from the action of all lower temperatures, but such a shielding is beyond our laboratory expedients.

In watching the appearance of these special enhanced lines in stellar spectra we have a third series of phenomena available, and we find that the results are absolutely in harmony with what has gone before. Thus:

Gaseous stars..	Highest temperature..	Strong helium and faint enhanced lines.
Metallic stars..		Feeble helium and strong enhanced lines.
	Medium temperature..	
Carbon stars..		No helium and strong arc lines
	Lowest temperature..	Faint arc lines.

It is clear now, not only that the spectral changes in stars are associated with, or produced by, changes of temperature, but that the study of the enhanced spark and the arc lines lands us in the possibility of a rigorous stellar thermometry, such lines being more easy to observe than the relative lengths of spectrum.

Accepting this, we can take a long stride forward and, by carefully studying the chemical revelations of the spectrum, classify the stars along a line of temperature. But which line? Were all the stars in popular phraseology created hot? If so, we should simply deal with the running down of temperature, and because all the hottest stars are chemically alike, all cooler stars would be alike. But there are two very distinct groups of coolest stars; and since there are two different kinds of coolest stars, and only one kind of hottest stars, it cannot be merely a question either of a running up or a running down of temperature.

Many years of very detailed inquiry have convinced me that all stars save the hottest must be sorted out into two series—those getting hotter and those, like our sun, getting cooler, and that the hottest stage in the history of a star is reached near the middle of its life.

The method of inquiry adopted has been to compare large-scale photographs of the spectra of the different stars taken by my assistants at South Kensington; the complete harmony of the results obtained along various lines of other work carries conviction with it.

We find ourselves here in the presence of minute details exhibiting the workings of a chemical law, associated distinctly with temperature; and more than this, we are also in the presence of high temperature furnaces, entirely shielded by their vastness from the presence of those distracting phenomena which we are never free

from in the most perfect conditions of experiment we can get here.

What, then, is the chemical law? It is this: In the very hottest stars we deal with the gases hydrogen, helium, and doubtless others still unknown, almost exclusively. At the next lowest temperatures we find these gases being replaced by metals in the state in which they are observed in our laboratories when the most powerful jar-spark is employed. At a lower temperature still the gases almost disappear entirely, and the metals exist in the state produced by the electric arc. Certain typical stars showing these chemical changes may be arranged as follows:

Stars getting hotter.	Hottest stars.	Stars cooling.
	Bellatrix	
	ζ Tauri	β Persei
	Rigel	γ Lyra
	α Cygni	Castor
γ Cygni		Procyon
α Orionis		Arcturus and Sun

This, then, is the result of our first inquiry into the existence of the various chemical elements in the atmospheres of stars generally. We get a great diversity, and we know that this diversity accompanies changes of temperature. We have also found that the sun, which we independently know to be a cooling star, and Arcturus are identical chemically.

We have now dealt with the presence of the various chemical elements generally in the atmospheres of stars. The next point we have to consider is, whether the absorption which the spectrum indicates for us takes place from top to bottom of the atmosphere or only in certain levels.

In many of these stars the atmosphere may be millions of miles high. In each the chemical substances in the hottest and coldest portions may be vastly different. The region, therefore, in which this absorption takes place, which spectroscopically enables us to discriminate star from star, must be accurately known before we can obtain the greatest amount of information from our inquiries.

Our next duty then, clearly, is to study the sun—a star so near us that we can examine the different parts of its atmosphere, which we cannot do in the case of the more distant stars. By doing this we may secure facts which will enable us to ascertain in what parts of

the atmosphere the absorption takes place which produces the various phenomena on which the chemical classification has been based.

It is obvious that the general spectrum of the sun, like that of stars generally, is built up of all the absorptions which can make themselves felt in every layer of its atmosphere from bottom to top; that is, from the photosphere to the outermost part of the corona. Let me remind you that this spectrum is changeless from year to year.

Now, sun-spots are disturbances produced in the photosphere; and the chromosphere, with its disturbances, called prominences, lies directly above it. Here, then, we are dealing with the lowest part of the sun's atmosphere. We find first of all that, in opposition to the changeless general spectrum, great changes occur with the sun-spot period, both in the spots and chromosphere.

The spot spectrum is indicated, as was found in 1866, by the widening of certain lines; the chromospheric spectrum, as was found in 1868, by the appearance at the sun's limb of certain bright lines. In both cases the lines affected, seen at any one time, are relatively few in number.

In the spot spectrum, at a sun-spot minimum, we find iron lines chiefly affected; at a maximum they are chiefly of unknown or unfamiliar origin. At the present moment the affected lines are those recorded in the spectra of vanadium and scandium, with others never seen in a laboratory. That we are here far away from terrestrial chemical conditions is evidenced by the fact that there is not a gram of scandium available for laboratory use in the world at the present time.

Then we have the spectrum of the prominences and the chromosphere. That spectrum we are enabled to observe every day when the sun shines, as conveniently as we can observe that of sun spots. The chromosphere is full of marvels. At first, when our knowledge of spectra was very much more restricted than now, almost all the lines observed were unknown. In 1868 I saw a line in the yellow, which I found behaved very much like hydrogen, though I could prove that it was not due to hydrogen; for laboratory use the substance which gave rise to it I called helium. Next year I saw a line in the green at 1474 of Kirchhoff's scale. That was an unknown line, but in some subsequent researches I traced it to iron. From that day to this we have observed a large number of lines. They have

gradually been dragged out from the region of the unknown, and many are now recognized as enhanced lines, to which I have already called attention as appearing in the spectra of metals at a very high temperature.

But useful as the method of observing the chromosphere without an eclipse, which enables us

“ . . . to feel from world to world,”

as Tennyson has put it, has proved, we want an eclipse to see it face to face.

A tremendous flood of light has been thrown upon it by the use of large instruments constructed on a plan devised by Respighi and myself in 1871. These give us an image of the chromosphere painted in each one of its radiations, so that the exact locus of each chemical layer is revealed. One of the instruments employed during the Indian eclipse of this year is that used in photographing the spectra of stars, so that it is now easy to place photographs of the spectra of the chromosphere obtained during a total eclipse and of the various stars side by side.

I have already pointed out that the chemical classification indicated that the stars next above the sun in temperature are represented by γ Cygni and Procyon, one on the ascending, the other on the descending branch of the temperature curve.

Studying the spectra photographed during the eclipse of this year we see that practically the lower part of the sun's atmosphere, if present by itself, would give us the lines which specialize the spectra of γ Cygni or Procyon.

I recognize in this result a veritable Rosetta stone, which will enable us to read the celestial hieroglyphics presented to us in stellar spectra, and help us to study the spectra and to get at results much more distinctly and certainly than ever before.

One of the most important conclusions we draw from the Indian eclipse is that, for some reason or other, the lowest, hottest part of the sun's atmosphere does not write its record among the lines which build up the general spectrum so effectively as does a higher one.

There was another point especially important on which we hoped for information, and that was this: Up to the employment of the prismatic camera insufficient attention had been directed to the fact

that in observations made by an ordinary spectroscope no true measure of the height to which the vapors or gases extended above the sun could be obtained; early observations, in fact, showed the existence of glare between the observer and the dark moon; hence it must exist between us and the sun's surroundings.

The prismatic camera gets rid of the effects of this glare, and its results indicate that the effective absorbing layer—that, namely, which gives rise to the Fraunhofer lines—is much more restricted in thickness than was to be gathered from the early observations.

We are justified in extending these general conclusions to all the stars that shine in the heavens.

So much, then, in brief, for solar teachings in relation to the record of the absorption of the lower parts of stellar atmospheres.

Let us next turn to the higher portions of the solar surroundings, to see if we can get any effective help from them.

In this matter we are dependent absolutely upon eclipses, and I shall fulfill my task very badly if I do not show you that the phenomena then observable when the so-called corona is visible, full of awe and grandeur to all, are also full of precious teaching to the student of science. This also varies like the spots and prominences with the sun-spot period.

It happened that I was the only person that saw both the eclipse of 1871 at the maximum of the sun-spot period and that of 1878 at minimum; the corona of 1871 was as distinct from the corona of 1878 as anything could be. In 1871 we got nothing but bright lines, indicating the presence of gases; namely, hydrogen and another, since provisionally called coronium. In 1878 we got no bright lines at all, so I stated that probably the changes in the chemistry and appearance of the corona would be found to be dependent upon the sun-spot period, and recent work has borne out that suggestion.

I have now specially to refer to the corona as observed and photographed this year in India by means of the prismatic camera, remarking that an important point in the use of the prismatic camera is that it enables us to separate the spectrum of the corona from that of the prominences.

One of the chief results obtained is the determination of the position of several lines of probably more than one new gas, which, so far, have not been recognized as existing on the earth.

Like the lowest hottest layer, for some reason or other, this upper layer does not write its record among the lines which build up the general spectrum.

GENERAL RESULTS REGARDING THE LOCUS OF ABSORPTION
IN STELLAR ATMOSPHERES

We learn from the sun, then, that the absorption which defines the spectrum of a star is the absorption of a middle region, one shielded both from the highest temperature of the lowest reaches of the atmosphere, where most tremendous changes are continually going on and the external region where the temperature must be low, and where the metallic vapors must condense.

If this is true for the sun it must be equally true for Arcturus, which exactly resembles it. I go further than this, and say that in the presence of such definite results as those I have brought before you it is not philosophical to assume that the absorption may take place at the bottom of the atmosphere of one star or at the top of the atmosphere of another. The *onus probandi* rests upon those who hold such views.

So far I have only dealt in detail with the hotter stars, but I have pointed out that we have two distinct kinds of coolest ones, the evidence of their much lower temperature being the shortness of their spectra. In one of these groups we deal with absorption alone, as in those already considered; we find an important break in the phenomena observed; helium, hydrogen, and metals have practically disappeared, and we deal with carbon absorption alone.

But the other group of coolest stars presents us with quite new phenomena. We no longer deal with absorption alone, but accompanying it we have radiation, so that the spectra contain both dark lines and bright ones. Now, since such spectra are visible in the case of new stars, the ephemera of the skies, which may be said to exist only for an instant relatively, and when the disturbance which gives rise to their sudden appearance has ceased, we find their places occupied by nebulae, we cannot be dealing here with stars like the sun, which has already taken some millions of years to slowly cool, and requires more millions to complete the process into invisibility.

The bright lines seen in the large number of permanent stars which

resemble these fleeting ones—new stars, as they are called—are those discerned in the once mysterious nebulæ which, so far from being stars, were supposed not many years ago to represent a special order of created things.

Now the nebulæ differ from stars generally in the fact that in their spectra we have practically to deal with radiation alone; we study them by their bright lines; the conditions which produce the absorption by which we study the chemistry of the hottest stars are absent.

A NEW VIEW OF STARS

Here, then, we are driven to the perfectly new idea that some of the cooler bodies in the heavens, the temperature of which is increasing and which appear to us as stars, are really disturbed nebulæ.

What, then, is the chemistry of the nebulæ? It is mainly gaseous; the lines of helium and hydrogen and the flutings of carbon, already studied by their absorption in the groups of stars to which I have already referred, are present as bright ones.

The presence of the lines of the metals iron, calcium, and probably magnesium, shows us that we are not dealing with gases merely.

Of the enhanced metallic lines there are none; only the low temperature lines are present, so far as we yet know. The temperature, then, is low, and lowest of all in those nebulæ where carbon flutings are seen almost alone.

A NEW VIEW OF NEBULÆ

Passing over the old views, among them one that the nebulæ were holes in something dark which enabled us to see something bright beyond, and another that they were composed of a fiery fluid, I may say that not long ago, they were supposed to be masses of gases only, existing at a very high temperature.

Now, since gases may glow at a low temperature as well as at a high one, the temperature evidence must depend upon the presence of cool metallic lines and the absence of the enhanced ones.

The nebulæ, then, are relatively cool collections of some of the permanent gases and of some cool metallic vapors, and both gases and metals are precisely those I have referred to as writing their records most visibly in stellar atmosphere.

Now, can we get more information concerning this association of certain gases and metals? In laboratory work it is abundantly recognized that all meteorites (and many minerals) when slightly heated give out permanent gases, and under certain conditions the spectrum of the nebulae may in this way be closely approximated to. I have not time to labor this point, but I may say that a discussion of all the available observations to my mind demonstrates the truth of the suggestion, made many years ago by Professor Tait before any spectroscopic facts were available, that the nebulae are masses of meteorites rendered hot by collisions.

Surely human knowledge is all the richer for this indication of the connection between the nebulae, hitherto the most mysterious bodies in the skies, and the "stones that fall from heaven."

CELESTIAL EVOLUTION

But this is, after all, only a stepping stone, important though it be. It leads us to a vast generalization. If the nebulae are thus composed, they are bound to condense to centers, however vast their initial proportions, however irregular the first distribution of the cosmic clouds which compose them. Each pair of meteorites in collision puts us in mental possession of what the final stage must be. We begin with a feeble absorption of metallic vapors round each meteorite in collision; the space between the meteorites is filled with the permanent gases driven out farther afield and having no power to condense. Hence dark metallic and bright gas lines. As time goes on the former must predominate, for the whole swarm of meteorites will then form a gaseous sphere with a strongly heated center, the light of which will be absorbed by the exterior vapor.

The temperature order of the group of stars with bright lines as well as dark ones in their spectra has been traced, and typical stars indicating the chemical changes have been as carefully studied as those in which absorption phenomena are visible alone, so that now there are no breaks in the line connecting the nebulae with the stars on the verge of extinction.

Here we are brought to another tremendous outcome—that of the evolution of all cosmical bodies from meteorites, the various stages recorded by the spectra being brought about by the various conditions which follow from the conditions.

These are, shortly, that at first collisions produce luminosity among the colliding particles of the swarm, and the permanent gases are given off and fill the interspaces. As condensation goes on, the temperature at the center of condensation always increasing, all the meteorites in time are driven into a state of gas. The meteoritic bombardment practically now ceases for lack of material, and the future history of the mass of gas is that of a cooling body, the violent motions in the atmosphere while condensation was going on now being replaced by a relative calm.

The absorption phenomena in stellar spectra are not identical at the same mean temperature on the ascending and descending sides of the curve, on account of the tremendous difference in the physical conditions.

In a condensing swarm, the center of which is undergoing meteoritic bombardment from all sides, there cannot be the equivalent of the solar chromosphere; the whole mass is made up of heterogeneous vapor at different temperatures and moving with different velocities in different regions.

In a condensed swarm, of which we can take the sun as a type, all action produced from without has practically ceased; we get relatively a quiet atmosphere and an orderly assortment of the vapors from top to bottom, disturbed only by the fall of condensed metallic vapors. But still, on the view that the differences in the spectra of the heavenly bodies chiefly represent differences in degree of condensation and temperature, there can be *au fond*, no great chemical difference between bodies of increasing and bodies of decreasing temperature. Hence we find at equal mean temperatures on opposite sides of the temperature curve this chemical similarity of the absorbing vapors proved by many points of resemblance in the spectra, especially the identical behavior of the enhanced metallic and cleveite lines.

CELESTIAL DISSOCIATION

The time you were good enough to put at my disposal is now exhausted, but I cannot conclude without stating that I have not yet exhausted all the conceptions of a high order to which Fraunhofer's apparently useless observation has led us.

The work which to my mind has demonstrated the evolution of the

cosmos as we know it from swarms of meteorites, has also suggested a chemical evolution equally majestic in its simplicity.

A quarter of a century ago I pointed out that all the facts then available suggested the hypothesis that in the atmospheres of the sun and stars various degrees of "celestial dissociation" were at work, a "dissociation" which prevented the coming together of the finest particles of matter which at the temperature of the earth and at all artificial temperature yet attained here compose the metals, the metal-loids and compounds.

On this hypothesis the so-called atoms of the chemist represent not the origins of things, but only early stages of the evolutionary process.

At the present time we have tens of thousands of facts which were not available twenty-five years ago. All these go to the support of the hypothesis, and among them I must indicate the results obtained at the last eclipse, dealing with the atmosphere of the sun in relation to that of the various stars of higher temperature to which I called your attention. In this way we can easily explain the enhanced lines of iron existing practically alone in Alpha Cygni. I have yet to learn any other explanation.

I have nothing to take back, either from what I then said or what I have said since on this subject, and although the view is not yet accepted, I am glad to know that many other lines of work which are now being prosecuted tend to favor it.

I have no hesitation in expressing my conviction that in a not distant future the inorganic evolution to which we have been finally led by following up Fraunhofer's useless experiment will take its natural place side by side with that organic evolution, the demonstration of which has been one of the glories of the nineteenth century.

And finally now comes the moral of my address. If I have helped to show that observations having no immediate practical bearing may yet help on the thought of mankind, and that this is a thing worth the doing, let me express a hope that such work shall find no small place in the future University of Birmingham.

XXXVI

ROBERT KOCH

1843-1910

Robert Koch, born at Klausthal, Hanover, Germany, December 11, 1843, graduated from Göttingen in 1866. After a short period as assistant surgeon in the General Hospital in Hamburg, he practised medicine at Langenhagen, Kackwitz, and Wollstein from 1872 to 1880, during which time he began his researches in bacteriology. By 1878 he had placed bacteriology on a scientific basis. In 1880 he was called to Berlin as chief of the Sanitary Institute, where he continued his studies of tuberculosis and cholera. After inventing new microscopical appliances and a new technique, in 1882 he stated his discovery of the tubercle bacillus. In 1883, after publishing a method for the prevention of anthrax by inoculation, he was sent by his government to Egypt and India to investigate cholera. During that work he discovered the cholera bacillus. In 1884 he returned to Germany and in the following year went to France as cholera commissioner. In 1888 he published a paper on the prophylaxis of infectious diseases in the army. In later years he investigated the bubonic plague, malaria, and sleeping-sickness. He died at Baden-Baden, May 28, 1910.

THEORY OF BACTERIA *

I am well aware that the investigations above described are very imperfect. It was necessary, in order to have time for those parts of the investigation which seemed the most important and essential, to omit the examination of many organs, such as the brain, heart, retina, etc., which ought not to pass unnoticed in researches on infective diseases. For the same reason no record was kept of the temperature,

* From the English translation (1880) of *Untersuchungen über die Aetiologie der Wundinfektionskrankheiten* (1878).

although this would undoubtedly have yielded most interesting results. I have intentionally refrained from entering into details of morbid anatomy, as only the etiology interested me, and as I did not feel qualified to undertake a study of the morbid anatomy of traumatic infective diseases. I must therefore leave this part of the investigation to those who are better able to undertake it.

Nevertheless I consider that the results of my researches are sufficiently definite to enable me to deduce from them some well founded conclusions.

In this summary I shall, however, confine myself to the most obvious conclusions. It has indeed of late become too common to draw the most sweeping conclusions as to infective diseases in general from the most unimportant observations on bacteria. I shall not follow this custom, although the material at my command would furnish rich food for meditation. For the longer I study infective diseases the more am I convinced that generalisations of new facts are here a mistake, and that every individual infective disease or group of closely allied diseases must be investigated for itself.

As regards the artificial traumatic infective diseases observed by me, the conditions which must be established before their parasitic nature can be proved, we completely fulfilled in the case of the first five, but only partially in that of the sixth. For the infection was produced by such small quantities of fluid (blood, serum, pus, etc.) that the result cannot be attributed to a merely chemical poison.

In the materials used for inoculation bacteria were without exception present, and in each disease a different and well marked form of organism could be demonstrated.

At the same time, the bodies of those animals which died of the artificial traumatic infective diseases contained bacteria in such numbers that the symptoms and the death of the animals were sufficiently explained. Further, the bacteria found were identical with those which were present in the fluid used for inoculation, and a definite form of organisms corresponded in every instance to a distinct disease.

These artificial traumatic infective diseases bear the greatest resemblance to human traumatic infective diseases, both as regards their origin from putrid substances, their course, and the result of post-mortem examination. Further, in the first case, just as in the last,

the parasitic organisms could be only imperfectly demonstrated by the earlier methods of investigation; not till an improved method of procedure was introduced was it possible to obtain complete proof that they were parasitic diseases. We are therefore justified in assuming that human traumatic infective diseases will in all probability be proved to be parasitic when investigated by these improved methods.

On the other hand, it follows from the fact that a definite pathogenic bacterium, e. g., the septicæmic bacillus, cannot be inoculated on every variety of animal (a similar fact is also true with regard to the bacillus anthracis); that the septicæmia of mice, rabbits, and man are not under all circumstances produced by the same bacterial form. It is of course possible that one or other of the bacteric forms found in animals also play a part in such diseases in the human subject. That, however, must be especially demonstrated for each case; *a priori* one need only expect that bacteria are present; as regards form, size and conditions of growth, they may be similar, but not always the same, even in what appear to be similar diseases in different animals.

Besides the pathogenic bacteria already found in animals there are no doubt many others. My experiments refer only to those diseases which ended fatally. Even these are in all probability not exhausted in the six forms mentioned. Further experiments on many different species of animals, with the most putrid substances and with every possible modification in the method of application, will doubtless bring to light a number of other infective diseases, which will lead to further conclusions regarding infective diseases and pathogenic bacteria.

But even in the small series of experiments which I was able to carry out, one fact was so prominent that I must regard it as constant, and, as it helps to remove most of the obstacles to the admission of the existence of a centagium vivum for traumatic infective diseases, I look on it as the most important result of my work. I refer to the differences which exist between pathogenic bacteria and to the constancy of their characters. A distinct bacteric form corresponds, as we have seen, to each disease, and this form always remains the same, however often the disease is transmitted from one animal to another. Further, when we succeed in reproducing the same disease *de novo* by the injection of putrid substances, only the same bacteric form occurs which was before found to be specific for that disease.

Further, the differences between these bacteria are as great as could

be expected between particles which border on the invisible. With regard to these differences, I refer not only to the size and form of the bacteria, but also to the conditions of their growth, which can be best recognized by observing their situation and grouping. I therefore study not only the individual alone, but the whole group of bacteria, and would, for example, consider a micrococcus which in one species of animal occurred only in masses (i. e., in a zooglæa form), as different from another which in the same variety of animal, under the same conditions of life, was only met with as isolated individuals. Attention must also be paid to the physiological effect, of which I scarcely know a more striking example than the case of the bacillus and the chain-like micrococcus growing together in the cellular tissue of the ear; the one passing into the blood and penetrating into the white blood corpuscles, the other spreading out slowly into the tissues in its vicinity and destroying everything around about; or again, the case of the septicæmic and pyæmic micrococci of the rabbit in their different relations to the blood; or lastly, the bacilli only extending over the surface of the aural cartilage in the erysipetalous disease, as contrasted with the bacillus anthracis, likewise inoculated on the rabbit's ear, but quickly passing into the blood.

As, however, there corresponds to each of the diseases investigated a form of bacterium distinctly characterized by its physiological action, by its conditions of growth, size, and form, which, however often the disease be transmitted from one animal to another, always remains the same and never passes over into another form, e. g., from the spherical to the rod shaped, we must in the meantime regard these different forms of pathogenic bacteria as distinct and constant species.

This is, however, an assertion that will be much disputed by botanists, to whose special province this subject really belongs.

Amongst those botanists who have written against the subdivision of bacteria into species, is Nägeli, who says, "I have for ten years examined thousands of different forms of bacteria, and I have not yet seen any absolute necessity for dividing them even into two distinct species."

Brefeld also states that he can only admit the existence of specific forms justifying the formation of distinct species when the whole history of development has been traced by cultivation from spore to spore in the most nutritive fluids.

Although Brefeld's demand is undoubtedly theoretically correct it cannot be made a *sine qua non* in every investigation on pathogenic bacteria. We should otherwise be compelled to cease our investigations into the etiology of infective diseases till botanists have succeeded in finding out the different species of bacteria by cultivation and development from spore to spore. It might then very easily happen that the endless trouble of pure cultivation would be expended on some form of bacterium which would finally turn out to be scarcely worthy of attention. In practice only the opposite method can work. In the first place certain peculiarities of a particular form of bacterium different from those of other forms, and in the second place its constancy, compel us to separate it from other less known and less interesting, and provisionally to regard it as a species. And now, to verify this provisional supposition, the cultivation from spore to spore may be undertaken. If this succeeds under conditions which cut out all sources of fallacy, and if it furnishes a result corresponding to that obtained by the previous observations, then the conclusions which were drawn from these observations and which led to its being ranked as a distinct species must be regarded as valid.

On this, which as it seems to me is the only correct practical method, I take my stand, and, till the cultivation of bacteria from spore to spore shows that I am wrong, I shall look on pathogenic bacteria as consisting of different species.

In order, however, to show that I do not stand alone in this view, I shall here mention the opinion of some botanists who have already come to a similar conclusion.

Cohn states that, in spite of the fact that many dispute the necessity of separating bacteria into genera or species, he must nevertheless adhere to the method as yet followed by him, and separate bacteria of a different form and fermenting power from each other, so long as complete proof of their identity is not given.

From his investigations on the effects of different temperatures and of desiccation on the development of bacterium termo, Eidam came to the conclusion that different forms of bacteria require different conditions of nutriment, and that they behave differently towards physical and chemical influences. He regards these facts as a further proof of the necessity of dividing organisms into distinct species.

I shall bring forward another reason to show the necessity of look-

ing on the pathogenic bacteria which I have described as distinct species. The greatest stress, in investigations on bacteria, is justly laid on the so-called pure cultivations, in which only one definite form of bacterium is present. This evidently arises from the view that if, in a series of cultivations, the same form of bacterium is always obtained, a special significance must attach to this form: it must indeed be accepted as a constant form, or in a word as a species. Can, then, a series of pure cultivations be carried out without admixture of other bacteria? It can in truth be done, but only under very limited conditions. Only such bacteria can be cultivated pure, with the aids at present at command, which can always be known to be pure, either by their size and easily recognizable form, as the bacillus anthracis, or by the production of a characteristic coloring matter as the pigment bacteria. When, during a series of cultivations, a strange species of bacteria has by chance got in, as may occasionally happen under any circumstances, it will in these cases be at once observed, and the unsuccessful experiment will be thrown out of the series without the progress of investigation being thereby necessarily interfered with.

But the case is quite different when attempts are made to carry out cultivations of very small bacteria, which, perhaps, cannot be distinguished at all without staining; how are we then to discover the occurrence of contamination? It is impossible to do so, and therefore all attempts at pure cultivation in apparatus, however skilfully planned and executed, must, as soon as small bacteria with but little characteristic appearances are dealt with, be considered as subject to unavoidable sources of fallacy, and in themselves inconclusive.

But nevertheless a pure cultivation is possible, even in the case of the bacteria which are smallest and most difficult to recognise. This, however, is not conducted in cultivation apparatus, but in the animal body. My experiments demonstrate this. In all the cases of a distinct disease, e. g., of septicæmia of mice, only the small bacilli were present, and no other form of bacterium was ever found with it, unless in the case where that causing the tissue gangrene was intentionally inoculated at the same time. In fact, there exists no better cultivation apparatus for pathogenic bacteria than the animal body itself. Only a very limited number of bacteria can grow in the body, and the penetration of organisms into it is so difficult that the uninjured living body may be regarded as completely isolated with respect to other forms of

bacteria than those intentionally introduced. It is quite evident, from a careful consideration of the two diseases produced in mice—septicæmia and gangrene of the tissue—that I have succeeded in my experiments in obtaining a pure cultivation. In the putrefying blood, which was the cause of these two diseases, the most different forms of bacteria were present, and yet only two of these found in the living mouse the conditions necessary for their existence. All the others died, and these two alone, a small bacillus and a chain-like micrococcus, remained and grew. These could be transferred from one animal to another as often as was desired, without suffering any alteration in their characteristic form, in their specific physiological action and without any other variety of bacteria at any time appearing. And further, as I have demonstrated, it is quite in the power of the experimenter to separate these two forms of bacteria from each other. When the blood in which only the bacilli are present is used, these alone are transmitted, and thenceforth are obtained quite pure; while on the other hand, when a field mouse is inoculated with both forms of bacteria, the bacilli disappear, and the micrococcus can be then cultivated pure. Doubtless an attempt to unite these two forms again in the same animal by inoculation would have been successful. In short, one has it completely in one's power to cultivate several varieties of bacteria together, to separate them from each other, and eventually to combine them again. Greater demands can hardly be made on a pure cultivation, and I must therefore regard the successive transmission of artificial infective diseases as the best and surest method of pure cultivation. And it can further claim the same power of demonstrating the existence of specific forms of bacteria, as must be conceded to any faultless cultivation experiments.

From the fact that the animal body is such an excellent apparatus for pure cultivation, and that, as we have seen, when the experiments are properly arranged and sufficient optical aids used, only one specific form of bacterium can be found in each distinct case of artificial traumatic infective disease, we may now further conclude that when, in examining a traumatic infective disease, several different varieties of bacteria are found, as e. g., chains of small granules, rods, and long, oscillating threads—such as were seen together by Coze and Feltz in the artificial septicæmia of rabbits—we have to do either with a combined infective disease,—that is, not a pure one,—or, what in the case

cited is more probable, an inexact and inaccurate observation. When, therefore, several species of bacteria occur together in any morbid process, before definite conclusions are drawn as to the relations of the disease in question to the organisms, either proof must be furnished that they are all concerned in the morbid process, or an attempt must be made to isolate them and to obtain a true pure cultivation. Otherwise we cannot avoid the objection that the cultivation was not pure, and therefore not conclusive. I shall only briefly refer to a further necessary consequence of the admission of the existence of different species of pathogenic bacteria. The number of the species of these bacteria is limited; for, of the numerous diverse forms present in putrid fluids, one or but few can in the most favorable cases develop in the animal body. Those which disappear are, for that species of animal at least, not pathogenic bacteria. If, however, as follows from the foregoing, there exist hurtful and harmless bacteria, experiments performed on animals with the latter, e. g., with bacterium termo, prove absolutely nothing for or against the behavior of the former—the pathogenic—forms. But almost all the experiments of this nature have been carried out with the first mixture of different species of bacteria which came to hand without there being any certainty that pathogenic bacteria were in reality present in the mixture. It is therefore evident that none of these experiments can be regarded as furnishing evidence of any value for or against the parasitic nature of infective diseases.

In all my experiments, not only have the form and size of the bacteria been constant, but the greatest uniformity in their actions on the animal organisms has been observed, though no increase of virulence, as described by Coze and Feltz, Davaine, and others. This leads me to make some remarks on the supposed law of the increasing virulence of blood when transmitted through successive animals, discovered or confirmed by the investigators just named.

The discovery of this law has, as is well known, been received with great enthusiasm, and it has excited no little interest owing to its intimate bearing on the doctrine of natural selection (*Anpassung* and *Vererbung*). Some investigators, who are in other things very exact, have allowed themselves to be blinded by the seductive theory that the insignificant action of a single putrefactive bacterium may, by continued natural selection in passing from animal to animal, be increased

in virulence till it becomes deadly though a drop of the infective liquid be diluted in a quadrillion times. They have founded thereon the most beautiful practical applications, not suspecting that the bacteria in question have never been certainly demonstrated.

The original works of Coze and Feltz, as also that of Davaine, are not at my disposal for reference; and I cannot therefore enter into a complete criticism of them. So far, however, as I can gather from the references accessible to me, especially from the detailed notices in Virchow and Hirsch's "Jahnesbericht," no complete proof that the virulence of septicæmic blood increases from generation to generation seems to have been furnished. Apparently blood more and more diluted was injected, and astonishment was felt when this always acted, the effect being then ascribed to its increasing virulence. But controlling experiments to ascertain whether the septicæmic blood were not already as virulent in the second and third generations as in the twenty-fifth, do not seem to have been made. My experiments so far support and are in accordance with those of Coze, Feltz, and Davaine in that for the first infection of an animal relatively large quantities of putrid fluid are necessary; but in the second generation, or at the latest in the third, the full virulence was attained, and afterwards remained constant.

Of my artificial infective diseases the septicæmia of the mouse has the greatest correspondence with the artificial septicæmia described by Davaine. If we were to experiment with this disease in the same manner as Davaine experimented, we would, if no controlling experiments were employed, find the same increase in virulence of the disease. It would only be necessary to use blood in slowly decreasing quantities in order to obtain in this way any progressive increase of the virulence that might be desired. I, however, took from the second or third animal the smallest possible quantity of material for inoculation, and thus arrived more quickly at the greatest degree of virulence. Till, therefore, I am assured that, in the septicæmia observed by Davaine, such controlling experiments were made, I can only look on an increase in virulence as holding good for the earlier generations. In order to explain this we do not, however, require to have recourse to the magical wand of natural selection; a feasible explanation can be very naturally furnished. Let us take again the septicæmia of mice, as being the most suitable example.

If two drops of putrefying blood be injected into such an animal there is introduced not only a number of totally distinct species of bacteria, but also a certain amount of dissolved putrid poison (sepsin), not sufficient to produce a fatal effect, but yet certainly not without influence on the health of the animal. Different factors must therefore be considered as affecting the health of the animal. On the one hand there is the dissolved poison, on the other the different species of bacteria, of which, however, perhaps only two, as in the example before us, can multiply in the body of the mouse and there exert a continuous noxious influence. Only one of these two species can penetrate into the blood, and if the blood alone be used for further inoculations, only this one variety will come victorious out of the battle for existence. The further development of the experiment depends entirely on the quantity of the putrid poison, and on the relation of the two forms of bacteria to each other in point of numbers. If one injects a large amount of septic poison and a large number of that variety of bacteria which increase locally (in this case the chain-like micrococci causing the gangrene of the tissue), but only a very small number of the bacteria which pass into the blood (here the bacilli), the first animal experimented on will die, as a result of the preponderation influence of the first two factors before many bacilli can have got into the blood and multiplied there. Of the blood of this first animal, containing, as it does, proportionately very few bacilli, one-fifth to one-tenth of a drop must be inoculated in order to convey the disease with certainty. In the second animal, however, only the bacilli are introduced, and these develop undisturbed in the blood. For the infection of the third animal the smallest quantity of this blood which can produce an effect is then sufficient, and after this third generation the virulence of the blood remains uniform.

We may also imagine another case in which the increase of the virulence may go on through more than two generations without any modification resulting from natural selection and transmission from animal to animal. This would take place if several species of bacteria capable of passing into the blood were introduced into the animal at the first injection. Let us suppose, for example, that in the same putrefying blood which served for the foregoing experiment, the bacilli of anthrax were also present, there would then be contained in the blood of the first animal not only the septicæmic bacillus, but also

bacillus anthracis, and of each only a small number; of the anthrax bacilli there would be even fewer than of the other, because in mice they are deposited chiefly in the spleen, lungs, etc.; while in the blood of the heart they are, even in the most favorable cases, only sparsely distributed. On the other hand, the anthrax bacilli have this advantage, that, provided they be inoculated in considerable numbers, they kill even within twenty hours, while the septicæmic bacilli only destroy life after fifty hours. In the blood of the second animal, therefore, both species of bacilli would be present in larger numbers than in the first, although not yet so numerous as if either organism had been inoculated singly. Hence a larger quantity of blood is necessary to ensure transmission to a third animal. Perhaps this might be the case even in the fourth generation, till finally one or other variety of bacillus would alone be present in the blood injected. Probably this would be the septicæmic bacillus.

In this way the experiments of Coze, Feltz, and Davaine may admit of simple explanation and be brought into harmony with my results.

